Essays on Labor and Development Economics

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Abstract

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This dissertation studies the impact of institutional interventions on labor markets in the United States, Norway and India. The labor markets studied are diverse, and include the criminal sector in the United States, the healthcare sector in Norway and the market for workfare employment in rural India.

Chapter 1 studies whether juvenile offenders are deterred by the threat of criminal sanctions. Existing research, which studies adolescent crime as a series of on-the-spot decisions, finds that deterrence estimates are negligible at best. This paper first presents a model that allows the return from crime to increase with previous criminal involvement. The predictions of the model are tested using policy variation in the United States over the period 2006-15. The results show that when criminal capital accumulates, juveniles may respond in anticipation of increases in criminal sanctions. Accounting for these anticipatory responses can overturn the conclusion that harsh sanctions do not deter juvenile crime.

Chapter 2 studies the impact of a graduate's first job on her career trajectory, and how job-seeking graduates respond to the persistence of these "first job effects". For identification, we exploit a natural experiment in Norway, where doctors' first jobs were allocated through a random serial dictatorship mechanism until 2013. We use administrative data on individual outcomes to confirm empirically that the residency allocation mechanism effectively randomized choice sets of hospitals across medical graduates. We then use the resulting variation in individual doctors choice sets to show that first jobs affect doctors' earnings, place of residence, and specialization in the long run.

Chapter 3 evaluates the effects of encouraging the selection of local politicians in India via community-based consensus, as opposed to a secret ballot election. I find that financial incentives aimed at encouraging consensus-based elections and discouraging political competition crowd in younger, more educated political representatives. However, these incentives also lead to worse governance as measured by a fall in local expenditure and regressive targeting of workfare employment. These results can be explained by the fact that community-based processes are prone to capture by the local elite, and need not improve the quality of elected politicians or governance.

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Chapter 1

Juvenile Crime and Anticipated Punishment*

Ashna Arora†

Abstract

Are juvenile offenders deterred by the threat of criminal sanctions? Recent research suggests that they are not. This conclusion is based on the finding that criminal behavior decreases only marginally as individuals cross the age of criminal majority, the age at which they are transferred from the juvenile to the more punitive adult criminal justice system. Using a model of criminal capital accumulation, I show theoretically that these small reactions close to the age threshold mask larger responses away from, or in anticipation of, the age threshold. I exploit recent policy variation in the United States to show evidence consistent with this prediction - arrests of 13-16-year-olds rise significantly for offenses associated with street gangs, including drug, homicide, robbery, theft, burglary and vandalism offenses, when the age of criminal majority is raised from seventeen to eighteen. In contrast, and consistent with previous work, I find that arrests of 17-year-olds do not increase systematically in response. I provide suggestive evidence that this null effect is likely due to a simultaneous increase in under-reporting of crime by 17-year-olds when the age of criminal majority is raised to eighteen. Last, I use a back-of-the-envelope calculation to show that for every 17-year-old diverted from adult punishment, jurisdictions bore social costs on the order of $65,000 due to the corresponding increase in juvenile offending. In sum, this paper demonstrates that when criminal capital accumulates, juveniles may respond in anticipation of increases in criminal sanctions, and accounting for these anticipatory responses can overturn the conclusion that harsh sanctions do not deter juvenile crime.

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1 Introduction

Recent research in economics and criminology suggests that the threat of punitive sanctions does not deter young offenders from engaging in crime (Chalfin & McCrary 2014). This finding has informed the public policy shift towards increasing rehabilitation efforts and reducing punitive sanctions for younger offenders. This shift is reflected in states across the U.S., many of which have recently increased the age of criminal majority - the age at which young delinquents are transferred to the adult criminal justice system.

The view that punitive sanctions do not deter young offenders is not supported by qualitative evidence. For instance, young offenders report consciously desisting from criminal activity close to the age of criminal majority, driven by the differences they perceive in the treatment of juvenile and adult criminals (Glassner *et al.* 1983, Hekman *et al.* 1983).[1] While this divergence may be driven by methodological differences, it may also be explained by two limitations of the empirical literature. One, adolescent crime is modeled as a series of on-the-spot decisions, with no dependence on previous criminal involvement. Two, if crime is underreported at a higher rate for juveniles (those below the age of criminal majority) than adults, previous estimates may be picking up the combined effect of deterrence and under-reporting.

This paper addresses both of these concerns. I first formalize a theoretical model in which individuals not only evaluate the costs and benefits of crime in each period, but also accumulate criminal capital as they commit crime. Each period, returns to crime increase with accumulated criminal capital and decrease in potential sanctions. When the age of criminal majority (henceforth, ACM) is raised from seventeen to eighteen, this framework predicts that individuals younger than seventeen should also increase criminal activity, not just 17-year-olds. This suggests that we may be able to deal with the issue of under-reporting, since we do not need to rely exclusively on estimates based on 17-year-old offending.[2]

[1] Similarly, law enforcement officials often voice concerns about the potential for heightened juvenile gang recruitment and violence in response to raising the age of criminal majority. For instance, see https://home.chicagopolice.org/community/gang-awareness/ and https://www.dnainfo.com/new-york/20170330/new-york-city/raise-the-age-juvenile-justice-16-17-year-old-charged-adults

[2] Focusing on 13-16-year-olds has the additional advantage of not being confounded by incapacitation effects. Since juvenile sentences are often shorter than adult sentences, reported increases in 17-year-old crime may be driven by reduced incapacitation, or shorter sentences. This confound does not affect 13-16-year-olds, who face identical sentences

I present evidence consistent with these predictions using recent variation in the ACM in the United States. To examine juvenile offending that benefits from criminal capital, I use crimes most commonly associated with street gangs, which provide an environment for juveniles in the U.S. to build criminal experience and access additional criminal opportunities. Using a difference-in-difference-in-difference framework, I show that arrest rates of 13-16-year-olds for these crimes increase significantly when the ACM is raised from seventeen to eighteen. Arrest rates for 17-year-olds do not increase significantly, consistent with previous work. I provide suggestive evidence that this may be due to a simultaneous increase in under-reporting of crime committed by 17-year-olds.

A back-of-the-envelope calculation shows that for every 17-year-old diverted from adult sanctions, jurisdictions bore social costs on the order of $65,000 due to the increase in juvenile offending. A comparison with existing estimates of the benefits of having fewer 17-year-olds with criminal records indicates that raising the ACM was likely a move in the right direction. However, my estimates suggest that raising the ACM can cause an increase in juvenile crime, particularly when we look for reactions in anticipation of the age threshold. This qualifies the conclusion that raising the ACM is always a good strategy. These estimates are of particular relevance today, as states like Connecticut, Illinois, Massachusetts and Vermont have introduced legislation to increase the ACM even further to twenty-one.

The theoretical framework used in this paper is motivated by research which shows that criminal experience increases the return to future offending (Bayer *et al.* 2009, Pyrooz *et al.* 2013, Carvalho & Soares 2016, Sviatschi 2017).[3] In each period, rational, forward-looking individuals weigh the costs and benefits of crime to maximize lifetime utility. Benefits include both the immediate return to crime and the increase in future return to crime (via the accumulation of criminal capital).

This framework generates two main predictions. First, criminal involvement will decrease as adolescents approach the ACM. This is because the value of criminal capital diminishes considerably once adolescents are treated as adults and face higher criminal sanctions. This decline in

after the ACM change.

[3]Juveniles may also lose human capital while incarcerated (Hjalmarsson 2008, Aizer & Doyle 2015), increasing the return to criminal capital and perpetuating long-term offending.

the net return to future offending causes criminal activity to decline even before adolescents have reached the ACM.[4] Second, when the ACM is raised from seventeen to eighteen, this framework predicts that all individuals below eighteen should increase criminal activity, not just 17-year-olds. This is because the value of criminal capital increases for each age group that faces an extended period of low sanctions. This increase in the net return to future offending causes criminal activity to increase among 17-year-olds, as well as individuals younger than seventeen.

In light of these predictions, I turn to the empirical analysis. As a first step, I use the National Longitudinal Survey of Youth (1997-2001) to document patterns of criminal involvement and gang-membership by age, separating states by their ACM. Cross-sectional variation in the ACM across states is used to provide evidence consistent with the two main predictions of the model. One, criminal involvement and gang membership (used as a proxy for criminal capital) decline as adolescents approach the ACM. Two, this decline starts at a later age in states that set the ACM at eighteen, as compared to those that set it at seventeen. These patterns are consistent with the model, but remain suggestive.

For the core of the empirical analysis, I use recent variation in the ACM in Connecticut, Massachusetts, New Hampshire and Rhode Island to estimate the causal impact of the ACM on adolescent crime. Estimates are based on a difference-in-difference-in-difference strategy, which leverages variation in the policy across age groups, states and time. I first show that the overall arrest rate for 13-17-year-olds increases when the ACM is raised from seventeen to eighteen. This increase is driven by offenses associated with a medium or high level of street gang involvement.[5] Second, arrest rates increase for each age group under seventeen; the estimate for 17-year-olds, however, does not increase significantly. Next, I examine offense-specific arrest rates, and find that juvenile arrests for drug, homicide, robbery, theft, burglary and vandalism increase by over fifteen per cent of the mean. Arrest rates for offenses that are not associated with street gangs, such as driving under the influence and liquor law violations, do not increase for any of the age groups

[4]Criminologists have hypothesized that offenders may desist from criminal activity as they approach the age of majority (Reid 2011). Abrams (2012) also documents reactions in anticipation of gun-law changes, rationalized by a model of forward-looking behavior in which individuals respond by not making investments related to a criminal career.

[5]These are identified using the FBI's 2015 National Gang Report, in which agencies identify crimes most commonly associated with street gangs, and include homicide, assault, robbery, theft, vandalism and drug offenses.

under eighteen. Finally, I examine demographic heterogeneity in response patterns and find that these effects are mainly driven by arrests of White (including Hispanic) male adolescents. This is consistent with effective treatment differing across race groups - if youth of color are disproportionately charged in adult courts (Juszkiewicz 2009), raising the ACM may change their incentives less than those of White youth. In sum, these results show that deterrence effects are not negligible, particularly for serious offending.[6]

I also provide suggestive evidence that the null effect on 17-year-olds may be due to a simultaneous increase in under-reporting of crime when the ACM is raised to eighteen. I show that reported crime increases sharply as individuals surpass the ACM, which varies across states within the U.S.[7] I use the National Incident Based Reporting System (NIBRS) data for the years 2006-14 to show that reported crime increases sharply at age seventeen in states that set the ACM at seventeen, while this increase appears at age eighteen in states that set the ACM at eighteen. This pattern shows up irrespective of whether we use arrests or offenses known to measure criminal activity, and even when we restrict attention to the most serious crimes. These findings are consistent with the fact that local law enforcement officials exercise discretion over how to handle offenders, and that additional requirements must be met to hold juveniles in custody including a strict 48 hour deadline to file charges.[8]

Deterrence estimates are likely to enter the calculus of state governments deciding where to set the ACM. Proponents of raising the ACM usually argue that crime rates will be lower in the long run because incarceration in juvenile facilities reduces recidivism. However, this benefit must be weighed against the costs of reduced deterrence, as documented in this paper. Further, juvenile incarceration is an expensive proposition, outstripping the costs of adult prison in the states under consideration by a factor of two or three.[9] A back-of-the-envelope calculation indicates that the increase in juvenile crime cost the average law enforcement jurisdiction around $340,000 in social costs, including both the costs of heightened offending and additional incarceration expenses. On

[6] This is consistent with Bushway *et al.* (2013)'s findings that seasoned offenders were more responsive to fluctuations in law enforcement practices (Oregon 2000 - 2005).

[7] This is analogous to the strategies employed in Costa *et al.* (2016) and Loeffler & Chalfin (2017).

[8] Greenwood (1995), Chalfin & McCrary (2014) also note that juveniles may be arrested at different rates than adults.

[9] For instance, in Connecticut and Massachusetts, the cost per inmate in juvenile facilities is three times that in adult facilities (Justice Policy Institute 2014).

the benefit side, the increase in the ACM meant that the average law enforcement agency subjected 5.4 fewer 17-year-olds to adult sanctions. Therefore, policymakers should evaluate whether diverting a 17-year-old from adult sanctions is worth $65,000 in benefits associated with the absence of a criminal record like lower recidivism and higher employment. Recent studies that report increased annual earnings of around $6,000 in response to the clearing of a criminal record indicate that this may well be the case (Chapin *et al.* 2014, Selbin *et al.* 2017). The takeaway that this paper seeks to highlight, however, is that the social costs associated with raising the ACM can be sizable, contrary to the findings of previous studies.

This paper contributes to the literature on whether sanctions can deter crime in general, and adolescent crime in particular. The evidence on whether harsh sanctions can deter crime is mixed (Nagin 2013, Chalfin & McCrary 2014, O' Flaherty & Sethi 2014). Past studies have shown that it is be possible to deter adult criminals - sentence enhancements in the U.S. were shown to deter crimes involving firearms and drunk driving (Abrams 2012, Hansen 2015), poor prison conditions were found to deter adult crime (Katz *et al.* 2003),[10] California's three strikes law reduced felony arrests among offenders with two strikes (Helland & Tabarrok 2007) and sentence enhancements in Italy were found to reduce adult recidivism (Drago *et al.* 2009). Levitt (1998) also showed that as individuals transition from the juvenile to the adult system, crime falls by more in states where the adult system is more punitive relative to the juvenile system, indicative of a deterrence effect.

However, more recent research on young offenders finds that the increase in sanction severity at the ACM does not deter crime. These studies leverage the discontinuity in sanction severity at the ACM (Hjalmarsson 2009, Hansen & Waddell 2014, Costa *et al.* 2016, Lee & McCrary 2017) or exploit variation in the ACM over time (Loeffler & Grunwald 2015b, Loeffler & Chalfin 2017, Damm *et al.* 2017) to identify deterrence effects.[11] Since these studies implicitly assume that the return to crime is independent of previous criminal experience, the only test for deterrence is whether offending rates for those above the ACM are lower than those below.[12] Further, if crime report-

[10] Shapiro (2007) and Drago *et al.* (2011) show, however, that poor prison conditions do not lower recidivism in the U.S. and Italy respectively.

[11] An exception is Oka (2009) who shows that juveniles in Japan reduced criminal offending in response to a reduction in the ACM.

[12] Damm *et al.* (2017) also test for role-model effects on age groups below the age of criminal *responsibility*, the age at which individuals are transferred from the social service system to the criminal justice system. However, individuals

ing increases once individuals cross the ACM, this test will lead to an underestimate of deterrence effects. This paper shows that accounting for changes in reporting behavior requires looking at cohorts away from the ACM to measure deterrence effects, and that these can be sizable.

This paper also seeks to contribute to the literature on how individuals think and behave in order to develop alternative approaches to criminal deterrence. These approaches include Cognitive Behavioral Therapy (CBT) which helps adolescents develop alternative ways of processing and reacting to information in order to reduce criminal activity (Heller *et al.* 2017). The Gang Resistance Education And Training (G.R.E.A.T.) program, implemented in middle schools across America, also employs CBT techniques and has been found to reduce gang involvement, but has not significantly reduced violent offending (Pyrooz 2013). While interventions like CBT target those who have not managed to extricate themselves from violent networks, I focus on the fact that some adolescents may already possess the forward-looking behavior associated with reduced automaticity. It is possible that these adolescents respond to the higher ACM by staying in gangs longer, and continuing to offend at higher rates until a later age.

The results of this paper also contribute to the broader literature on how individuals account for future events when making decisions. Within the crime literature and closely related to the mechanism discussed in this paper, Imai & Krishna (2004), Mocan *et al.* (2005) and Munyo (2015) show that the threat to future employment can serve as an effective deterrent for criminal activity. O'Flaherty (1998) shows that those who confront a long sequence of criminal opportunities will act differently from those who confront a single opportunity. Studies in public finance and labor economics also show that individuals react in anticipation of events like the exhaustion of unemployment benefits (Mortensen 1977, Lalive *et al.* 2006), job losses (Hendren 2016) and even access to higher education (Khanna 2016). My findings are also consistent with an extensive margin response - juveniles who wish to reduce offending may leave criminal lifestyles such as gang membership entirely, rather than continue on as gang members who reduce offending once they cross the age threshold.

The rest of this paper is organized into five sections. Section 2 provides background informa-

between the age of criminal *responsibility* and the age of *majority* in Denmark benefit from a number of sentencing policies and options not available for adults (Kyvsgaard 2004), which makes it difficult to compare to the treatment in the US setting. Oka (2009), however, finds deterrence effects for the age group immediately below the ACM in Japan.

tion on juvenile crime trends and law enforcement approaches to juvenile delinquency since the 1990s. Section 3 lays out a theoretical framework in which individuals accumulate criminal capital, and generates predictions on the response to changes in the ACM. Section 4 describes how these predictions are tested in the data. Section 5 exploits policy variation in the Northeastern states in the U.S. to show causal evidence consistent with the theoretical framework and presents a partial cost-benefit analysis. Section 6 concludes.

2 Setting and Data

This section provides a brief description of juvenile crime trends in the U.S., policy responses to these trends, and the data sets used in the empirical analysis. Policy changes in the Northeastern states in the U.S. are described at some length, because they are used to identify the impact of the ACM on juvenile offending. I also provide suggestive evidence that criminal activity is more likely to be recorded (and hence, observable to the researcher) if the offender in question is above the ACM. Accounting for this variation in observability is one of the key contributions of this paper.

Juvenile Crime: Trends & Policy Responses

The roots of the juvenile justice system in the U.S. can be traced back to the nineteenth century, when the desire to remove juveniles from overcrowded adult prisons led to the development of separate facilities for abandoned and delinquent juveniles, as well as alternative options like out-of-home placement and probation. The juvenile justice system in the U.S. today comprises of both separate facilities for housing juveniles as well as a separate system of juvenile courts, in which the focus is on protecting and rehabilitating youthful offenders, usually disbursed via the individualized attention of a judge (as opposed to a jury). Incarceration lengths are shorter and conditions are better in juvenile than adult facilities (Myers 2003, Lee & McCrary 2017). It is also easier to expunge or seal criminal records if the offense was committed as a juvenile (Litwok 2014).

However, there exists substantial variation in the definition of juveniles within the U.S. The age of criminal majority - the lowest age at which offenders can be treated as adults by the criminal justice system[13] - has varied considerably across time and space within the U.S. Table A.1 displays a complete list of states by the age of criminal majority in 2017, and whether it has had a different age of criminal majority in the past. While the majority of states set the ACM at seventeen or eighteen, the ACM has varied from nineteen in 1993 Wyoming to sixteen in Connecticut, New York and North Carolina in the 2010s. Recently, Connecticut, Illinois and Vermont have even proposed bills to raise the age of criminal majority to twenty-one.

Trends in juvenile crime help explain some of the variation in the ACM over time. Figure A.1 plots juvenile and arrest rates in the U.S. for the period 1980-2013. Noticing the sharp increase in juvenile arrest rates in the 1990s (a trend that was not mirrored by adult arrest rates) states began to "get tough on juvenile crime", passing laws that increased the severity of juvenile sanctions. Between 1992 and 1975, all but three states passed legislation easing the transfer of juveniles into the adult system, instituted mandatory minimum sentences for serious offenses, reduced juvenile record confidentiality, increased victim rights or simply raised the age of criminal majority (Snyder & Sickmund 2006). As shown in Table A.1, New Hampshire, Wisconsin and Wyoming lowered their ACMs during this period. However, the simultaneous enactment of policy changes in other states makes it hard to disentangle the effect of the ACM from the effect of all of these other policies. Since the identification assumptions necessary for a difference-in-difference analysis are unlikely to be satisfied in this context, the empirical analysis focuses on more recent changes in states' ACMs.

ACM Changes in the 2000s

This section describes recent changes to the ACM across states in the U.S. As Figure A.1 shows, juvenile crime rates have fallen consistently since the 1990s. This decline has lent support to the legislative push to raise the ACM in states that set it below eighteen. Many of these changes were also catalyzed by the passage of the 2003 Prison Rape Elimination Act (PREA), a federal law aimed at preventing sexual assault in prison facilities. The PREA goes into effect in 2018, and requires offenders under eighteen to be housed separately from adults in correctional facilities, irrespective

[13] Some states have statutory exclusion laws in place, which allow offenders younger than the ACM to be tried as adults for serious felonies like murder.

of the state's ACM. Naturally, this requirement will be more costly to implement in states that set the ACM below eighteen and incarcerate 16 and 17-year-olds along with older inmates in adult facilities.

The Northeastern states of Connecticut, Massachusetts, New Hampshire, New York, Rhode Island and Vermont provide an arguably ideal setting in which to study the impact of ACM changes. The first reason is that there existed tremendous heterogeneity in the ACM within these states in 2003. Connecticut and New York set the ACM at sixteen, Massachusetts and New Hampshire at seventeen, and Rhode Island and Vermont at eighteen.[14] Second, each of these states has introduced legislation to change the ACM since the passage of the PREA, and five have been successful. This lends credibility to the assumption that the actual timing of legislation passage was unrelated to local crime trends. Last, their geographical proximity makes it likely that unobserved factors are similar across the states.

Two other states recently raised the ACM - Illinois raised the age for misdemeanors in 2010 and for all felonies in 2014, while Mississippi raised the age for misdemeanors and some felonies in 2011. Three reasons prevent the inclusion of these states into the study sample. First, the law change is not identical to that of the Northeast, since the ACM is raised only for a subset of offenses each time. Second, data is unavailable for most agencies in Illinois. Third, traditional control groups are unavailable, since none of these states' neighbors introduced legislation to change the ACM during the study period. Therefore, I focus on the Northeastern states as the primary setting for the empirical analysis, and show that the main results are robust to the inclusion of additional states in Section 5.5.

Arrest and Offense Data: Proxies for Criminal Activity

Criminal activity is not directly observable, so researchers rely on proxies like arrest and offense data generated by local law enforcement agencies. A shared concern of papers that use such data is that many steps lie between the criminal offense and the generation of an official report (Loeffler

[14] A state's ACM is usually an artifact of the time period in which it established its juvenile justice system. For instance, New York set its ACM at sixteen in 1909, while other states settled upon higher ACMs over the ensuing decades.

& Chalfin 2017, Costa *et al.* 2016), such as the victim's decision to file an official report.[15] Official data cannot reflect, for instance, the amount of crime which is not reported to the police[16] or crime that goes unreported due to the discretionary practices of individual officers.

Studies examining the effects of age-based criminal sanctions particularly worry that offense and arrest reports are *more* likely to be generated if offenders are treated as adults by the criminal justice system.[17] This is because law enforcement officials must comply with additional supervisory requirements while juveniles are held in custody - unlike adults, juveniles cannot be dropped off at the local or county jail. Furthermore, juveniles can only be detained for forty eight hours while charges are filed in juvenile court. These additional costs make it less likely that juvenile offenders are officially arrested or charged, and therefore, less likely that their offenses are included in official crime statistics. This is problematic for studies that compare individuals on either side of the ACM, because reported crime will be higher for individuals that face lower incentives to commit crime (individuals above the ACM). If the drop in actual crime is largely offset by the increased probability of a crime being reported, we are likely to find very small deterrence estimates. The latter effect may even dominate the former, leading to a rise in reported crime exactly when the incentives to commit crime decrease. Costa *et al.* (2016) examine biases in criminal statistics by testing for discontinuous *increases* in crime as individuals surpass the age of criminal majority in Brazil. They find a significant increase in non-violent crimes by individuals just above the age threshold, which suggests that under-reporting falls once offenders can be charged criminally as adults. An analogous strategy is followed by Loeffler & Chalfin (2017), who show that arrests dip sharply for 16-year-olds in Connecticut, as they are transitioned from the adult to the juvenile justice system.

I use an analogous argument to provide evidence suggestive of reduced under-reporting at the ACM in the U.S. I show that reported crime increases sharply at age seventeen in states that set the ACM at seventeen, while this increase appears at age eighteen in states that set the ACM at

[15]How crime statistics are generated is also a long-standing concern in criminology - see Black (1970), Black (1971) and Smith & Visher (1981).

[16]The National Crime Victimization Surveys from 2006-10 reported that less than half of all violent victimizations are reported to the police. Moreover, crimes against victims in the age group 12 to 17 were most likely to go unreported.

[17]For instance, see Loeffler & Grunwald (2015a) and Loeffler & Chalfin (2017).

eighteen. Using monthly data at the law enforcement agency level for the years 2006-14, Panel A of Figure 6 displays the proportion of arrests attributable to each age group in states that set the ACM at seventeen. Panel B repeats this exercise for states that set the ACM at eighteen. The spike in recorded crime is striking as we transition from the age just before the threshold (sixteen or seventeen) to the age where individuals are treated as adults by the criminal justice system (seventeen or eighteen). This is suggestive of reduced under-reporting as individuals cross the age of criminal majority. Therefore, existing papers that compare juveniles with adults are likely to report an estimate of deterrence that is adulterated by the effect of reduced under-reporting.

What are possible workarounds to get at true measures of deterrence? One way to circumvent this issue is to use data that is less likely to be manipulated. For instance, Costa *et al.* (2016) study violent death rates around the ACM in Brazil as a proxy for involvement in violent crime. They argue that this is an improvement over police records because death certificates that include the probable cause of death are necessary for burial and mandated by the national government. They also highlight the main drawback of this measure - violent death rates may not be reflective of trends in other, less violent crimes. In a similar vein, some studies on crime in the U.S. use data on offenses instead of arrests (Loeffler & Chalfin 2017, Abrams 2012), since the latter are more likely to be affected by police officer behavior. However, the age-crime profile described above is true irrespective of whether crime is defined as arrests or offenses. Figure A.2 recreates the age-crime profile, using the proportion of offenses attributable to each age group instead of arrests. There is a clear spike in the proportion of offenses attributable to 18-year-olds in states that set the ACM at eighteen, but not in states that set the ACM at seventeen. This indicates that data on offenders below the ACM (not just arrestees) may suffer from under-reporting as well. Therefore, using offense data provides a partial solution to the misreporting issue.

This paper proposes an alternative method to estimate deterrence effects. I examine responses among cohorts for whom the degree of under-reporting is held fixed. I test for responses to increases in the ACM among individuals who are always treated as juveniles, i.e. those to the left of the former ACM. Since these age groups are treated as juveniles both before and after the ACM change, the degree of under-reporting of crime is unchanged. If adolescents to the left of the threshold increase criminal activity when the ACM is moved further away from them, reported crime should increase. Furthermore, this response is a deterrence effect, since juveniles are responding to the expectation of lower sanctions in the future by increasing offending in the current period.

Street Gangs in the U.S. & Gang-Related Crime

This section uses criminological studies and national gang surveys to characterize youthful involvement in street gangs in the United States. Crimes most likely to be related to street gangs are the focus of the empirical analysis. The objective of this separation exercise is not to suggest that other crimes cannot react to the ACM change - in fact, they may react strongly if there is enough overlap between "gang" and "non-gang" crimes. Instead, the aim is to test whether the types of crime that fit the framework of criminal capital accumulation actually do respond to the ACM change.

Gangs[18] are a growing problem in the United States. Following a steady decline until the early 2000s, annual estimates of gang prevalence and gang-related violent, property and drug crimes have steadily increased (National Gang Center 2012, Egley *et al.* 2010).[19] Street gangs are central to the discussion of juvenile crime for two reasons. One, a large proportion of gang members are juveniles - the 2011 National Youth Gang Survey estimates that over a third of all gang members are under the age of eighteen, and Pyrooz & Sweeten (2015) estimate that there are over a million juvenile gang members in the U.S. today. Two, gang members contribute disproportionately to overall crime, particularly to violent adolescent crime. For instance, Thornberry (1998) and Fagan (1990) documented that while gang membership ranged from 14 to 30 per cent across six cities - Rochester, Seattle, Denver, San Diego, Los Angeles and Chicago - gang members contributed to at least sixty percent of drug dealing offenses and sixty percent of general delinquency and serious violence.[20]

Which crimes are most commonly associated with street gangs in the U.S.? Past work has

[18] The FBI National Crime Information Center defines a gang as three or more persons that associate for the purpose of criminal or illegal activity.

[19] Also see https://www.usnews.com/news/articles/2015/03/06/gang-violence-is-on-the-rise-even-as-overall-violence-declines

[20] Crime definitions varied by city. Recent research has also shown that this heightened delinquency cannot simply be attributed to individual selection effects (Barnes *et al.* 2010), and is likely to be associated with gang affiliation itself.

shown that gang members are not crime specialists (National Gang Center 2012, Thornberry 1998, Fagan 1990, Klein & L. Maxson 2010). This finding is confirmed by the FBI's 2015 National Gang Report, which collected information from law enforcement agencies about the degree of street gang involvement in various criminal activities.[21] I define gang-related offenses as those for which street gang involvement is reported as moderate or high in this report. These include eleven UCR offense categories - homicide, robbery, assault, burglary, theft (including motor vehicle theft), stolen property offenses, forgery and fraud, vandalism, weapon law violations and drug offenses.[22]

Street gangs in the U.S. provide an environment in which juveniles can accumulate criminal experience and access additional criminal opportunities, lending support to the assumptions of the theoretical framework. Additionally, previous involvement with law enforcement makes gang members more likely to be informed about changes in the ACM. These two features indicate that gang-related crime should react in line with the predictions of the model. Therefore, I use gang-related offenses to test the main predictions of the model. I also examine responses among offense categories with at most a low level of street gang involvement - arson, embezzlement, gambling, offenses against the family and children, driving under the influence and liquor laws, disorderly conduct (including drunkenness), and suspicion (including vagrancy and loitering).[23] The absence of an increase in "non-gang" crimes is used to rule out the hypothesis that general crime trends are driving the deterrence results found for "gang-related" crimes.

Data

Local law enforcement agencies in the United States choose to report crime statistics to federal agencies in one of two ways - the Uniform Crime Reports (UCR) and the National Incident Based Reporting System (NIBRS). This paper makes use of both of these data sources; the UCR covers more law enforcement agencies in the U.S., while the NIBRS presents a more detailed picture of

[21]The survey question asked respondents to indicate whether gang involvement in various criminal activities in their jurisdiction was High, Moderate, Low, Unknown or None.

[22]This crime pattern is broadly corroborated by Klein & L. Maxson (2010).

[23]I exclude from the empirical analysis the following offense categories - sex offenses, since the UCR definition of offenses classified as rape changed in 2013; runaways, a status offense which only applies to juveniles and would be expected to mechanically increase when 17-year-olds are treated as juveniles; uncategorized crimes, due to the lack of interpretability for these results. These three categories account for around 27% of total arrests.

crime within the agencies that it covers.

The Uniform Crime Reports have been compiled by the Federal Bureau of Investigation (FBI) since 1930. UCR data contain monthly data on criminal activity within the agency's jurisdiction, with subtotals by arrestee age and sex under each offense category.[24] As of 2015, law enforcement agencies representing over ninety per cent of the U.S. population have submitted their crime data via the UCR. This study uses monthly data at the law enforcement agency level for the six Northeastern states during 2006-15.

The National Incident Based Reporting System (NIBRS) collects information on each crime occurrence known to the police, and generates data as a by-product of local, state and federal automated records management systems. Importantly, offender profiles are generated independent of arrest using victim and witness statements. This allows us to examine separately whether reporting behavior, not just arrest behavior, is influenced by the age of the offender. As of 2012, law enforcement agencies representing twenty eight per cent of the population have submitted their crime data via the NIBRS.

To examine how juveniles accumulate criminal experience by offending and associating with delinquent peers, I use the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 is a nationally representative sample of approximately 9,000 youths who were twelve to sixteen years old as of December 31, 1996. This dataset includes self-reports on gang membership and criminal involvement (property, drug, assault and theft offenses) in the preceding twelve months for each year between 1997 and 2001. I use these responses as representative of the age at which the respondent spent the majority of the previous twelve months, and create age profiles for gang membership and criminal involvement, separating states by their ACM.[25]

3 Theoretical Framework

This section presents a model of criminal behavior in which individuals are aware of the existence of the ACM and internalize that current criminal activity increases the return to future criminal

[24] In an incident wherein multiple offenses were committed, only the crime that has the highest rank order in the list of ordered categories will be counted in the monthly totals.

[25] Pyrooz & Sweeten (2015) create gang membership by age profiles, but do not separate states by their ACM.

offending. This framework isolates a deterrence response by identifying cohorts that increase criminal activity in response to the change in the ACM, and then pinpoints cohorts for which under-reporting confounds are unlikely to be an issue.

Life-Cycle Model of Crime with an Anticipated Threshold

In Becker (1968)'s seminal framework, individuals undertake criminal activity if the benefits of crime outweigh the costs. I extend this model to allow individuals to accumulate criminal capital as they undertake criminal activity over their life course in a continuous time framework.[26] In line with recent work[27] criminal capital increases the return to future crime.[28] Individuals can only benefit from criminal capital by committing more crime in the future.

Adolescents are indexed by age t and have preferences that are represented by an intertemporally separable utility function $u(c_t, k_t, s_t)$. At each at age, adolescents decide how much criminal activity c_t to undertake, knowing that they will face criminal sanctions s_t if caught. The return to criminal activity is an increasing, concave function of criminal capital k_t.

$$u(c_t, k_t, s_t) = R(k_t).c_t - Prob(Caught).s_t$$

$$R_k \geq 0 \qquad R_{kk} \leq 0$$

$$c_t \geq 0$$

The probability of facing criminal sanctions $p(.)$ is assumed to be an increasing convex function of criminal activity c_t.[29]

[26]This is similar to the discrete time framework of Munyo (2015) in which both work- and crime-specific human capital evolve with past choices. Also related are Lee & McCrary (2017), who use a dynamic extension of Becker (1968) and the static model of time allocation by Grogger (1998) in which individuals allocate time between leisure, formal work and criminal activity. However, the return to crime is assumed to be independent of previous criminal involvement in both of these studies.

[27]Pyrooz *et al.* (2013) and Carvalho & Soares (2016) show that embeddedness and wages in gangs increase with participation in gang-related crime. Also see Levitt & Venkatesh (2000) who find that gang members are motivated by the symbolic value attached to upward mobility in drug gangs, as well as the tournament for future riches.

[28]This insight is also similar to that of the rational addiction literature, which argues that individual decision making reflects knowledge of inter-temporal complementarities in consumption. See Becker & Murphy (1988) for a theoretical exposition.

[29]This assumption is motivated by the fact that serious offenses are more likely to be reported to the police. For instance, the 2010 National Victimization Survey reports that less than 15 per cent of motor vehicle thefts were not reported to the police, while the analogous estimate for all other thefts was over 65 per cent.

$$u(c_t, k_t, s_t) = R(k_t).c_t - p(c_t).s_t$$

$$p_c \geq 0 \qquad p_{cc} \geq 0$$

Criminal activity adds to an individual's stock of criminal capital, which depreciates at the rate δ. Therefore, the change in criminal capital at each age is current criminal activity ("investment") less depreciation.

$$\dot{k_t} = c_t - \delta k_t$$

$$0 < \delta < 1$$

Sanctions s for criminal offenses are a function of age t, and increase sharply as adolescents surpass the ACM T.

$$s_t = \begin{cases} S_J & t < T \\ \\ S_A & t \geq T \end{cases} \qquad\qquad 0 < S_J < S_A$$

Individuals are forward-looking and maximize lifetime utility. Future flow utility is discounted at the rate $\rho \in (0, 1)$. The inter-temporal separability of the utility function allows us to write lifetime utility U_t as the discounted sum of flow utilities u_t.

$$U_t = \int_t^\infty e^{-\rho(\tau - t)} u(c_\tau, k_\tau, s_\tau) d\tau$$

At each age t, individuals choose how much crime to undertake c_t to maximize lifetime utility, subject to the criminal capital accumulation equation.

$$V_t = Max_{c_t} \int_t^\infty e^{-\rho(\tau - t)} u(c_\tau, k_\tau, s_\tau) d\tau$$

$$\text{s.t. } \dot{k_t} = c_t - \delta k_t$$

To solve this maximization problem, we first set up the current value Hamiltonian. Assume for now that sanctions s_t do not vary with t (or that $s = S_J = S_A$). The initial level of criminal capital k_0 is given.[30]

[30] k_0 determines the return to criminal activity for an individual with no criminal experience, and may be influenced by the criminal experience of one's peer group or access to criminal opportunities.

$$\mathcal{H}(c_t, k_t) = u(c_t, k_t, S_J) + \lambda_t(c_t - \delta k_t)$$

c_t, the control variable, can be chosen freely; k_t is the state variable, since its value is determined by past decisions; λ_t, the costate variable, is the shadow value of the state variable k_t. The Maximum Principle generates three conditions characterizing the optimum path for (c_t, k_t, λ_t):

$$\mathcal{H}_c = 0 \quad \Longrightarrow \quad R(k_t) - p_c(c_t)S_J + \lambda_t = 0 \qquad (2a)$$

$$\mathcal{H}_k = \rho\lambda_t - \dot{\lambda}_t \quad \Longrightarrow \quad R_k(k_t)c_t - \delta\lambda_t = \rho\lambda_t - \dot{\lambda}_t \qquad (2b)$$

$$lim_{t\to\infty} e^{-\rho t}\lambda_t k_t \leq 0 \qquad (2c)$$

Equation $(2a)$ pins down the optimal level of criminal activity at each age, and can be rewritten as

$$p_c(c_t)S_J = R(k_t) + \lambda_t$$

Individuals choose c_t to equate the marginal cost of crime $p_c(c_t)S_J$ with the marginal benefits of crime. Benefits from crime consist of the current return $R(k_t)$ plus the value of an additional unit of criminal capital in the future λ_t.

Equation $(2b)$ can be integrated to obtain the following expression

$$\lambda_t = \int_t^\infty e^{-(\rho+\delta)(\tau-t)} R_k(k_\tau)c_\tau d\tau$$

λ_t represents the shadow value of criminal capital k_t, and is equal to the present discounted value of future marginal returns to criminal capital. This implies that expectations about future decisions will influence the valuation of criminal capital in the current period. For instance, if criminal activity is expected to decrease in the future, λ_t will decrease even if returns to c_t are high in the current period t.

Equation $(2c)$ specifies that the value of criminal capital cannot accumulate at a rate faster than the discount rate on the optimal path. This ensures that optimizing individuals do not accumulate criminal capital that they do not intend to utilize.

Dynamics Under Fixed Sanctions

For simplicity, I fix $R(k_t) = k_t^\alpha$, $\alpha \in (0,1)$ and $p(c_t) = c_t^2$. Re-arranging the capital accumulation equation and first order conditions, dynamics in the model can be summarized by:

$$\dot{k_t} = c_t - \delta k_t = \tfrac{1}{2S_J}(k_t^\alpha + \lambda_t) - \delta k_t$$

$$\dot{\lambda_t} = (\rho + \delta)\lambda_t - \alpha c_t k_t^{\alpha-1} = (\rho + \delta - \tfrac{\alpha}{2S_J} k_t^{\alpha-1})\lambda_t - \tfrac{\alpha}{2S_J} k_t^{2\alpha-1}$$

Figure 1 displays the $\dot{k_t} = 0$ and $\dot{\lambda_t} = 0$ loci graphically.[31] The arrows show how k_t and λ_t must behave in order to satisfy conditions $(2a)$ and $(2b)$, given their initial values. The $\dot{k_t} = 0$ and $\dot{\lambda_t} = 0$ loci intersect at the steady state level of capital of criminal capital - optimizing individuals will not wish to increase or decrease their stock of criminal capital once they've accumulated $k = k_J^{SS}$. In the Appendix, I show that that the steady state level of k is given by

$$k_J^{SS} = [\tfrac{1}{2S_J\delta}\{\tfrac{\alpha}{(\rho+\delta)} + 1\}]^{\frac{1}{1-\alpha}}$$

The steady state value of criminal capital decreases in criminal sanctions S_J, depreciation rate δ and the rate at which future utility is discounted ρ; k_J^{SS} increases with the returns to additional criminal capital α.

This system of differential equations exhibits saddle path stability for a wide range of parameter values, described in detail in the Appendix.[32] Recall that the initial value of capital k_0 is assumed to be given, while the shadow value of capital λ_0 is free to adjust. Saddle path stability means that there is a unique value of λ_0 (on the saddle path, shown as the dashed line) such that k_t and λ_t converge to the steady state. If λ_0 starts below the saddle path, the individual eventually crosses into the region where both k_t and λ_t are falling indefinitely. If λ_0 starts above the saddle path, the individual eventually crosses into the region where both k_t and λ_t are rising indefinitely.

[31] This figure is drawn using the following parameter values: $\alpha = 0.4, \delta = 0.3, \rho = .05, s = 10$.

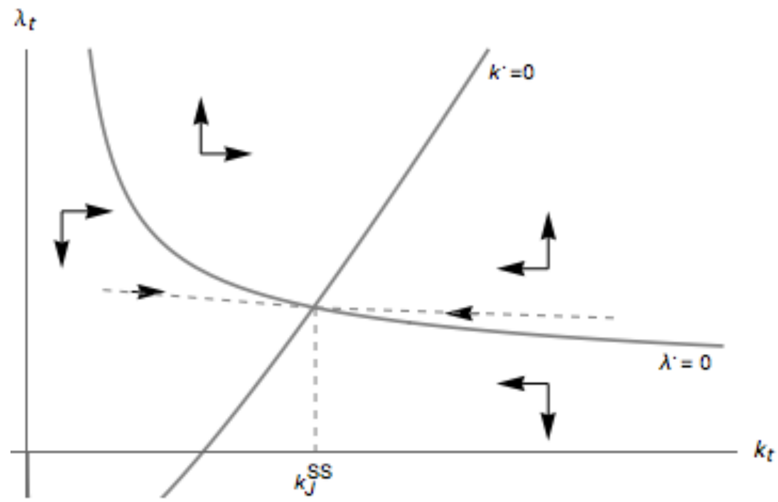
[32] For instance, $0 < \alpha \leq 0.5$ is a sufficient condition for saddle path stability.

Both of these cases will violate the transversality condition $(2c)$.[33]

Thus, given an initial value k_0, optimizing individuals will move along the saddle path towards k_J^{SS}. If an individual's initial k_0 is lower than the steady state k_J^{SS}, c_t and k_t will increase until $k_t = k_J^{SS}$, and criminal activity will stabilize at

$$c_J^{SS} = \frac{1}{2S_J}[(k_J^{SS})^\alpha + \lambda_J^{SS}]$$

FIGURE 1: SADDLE PATH UNDER AGE-INDEPENDENT SANCTIONS



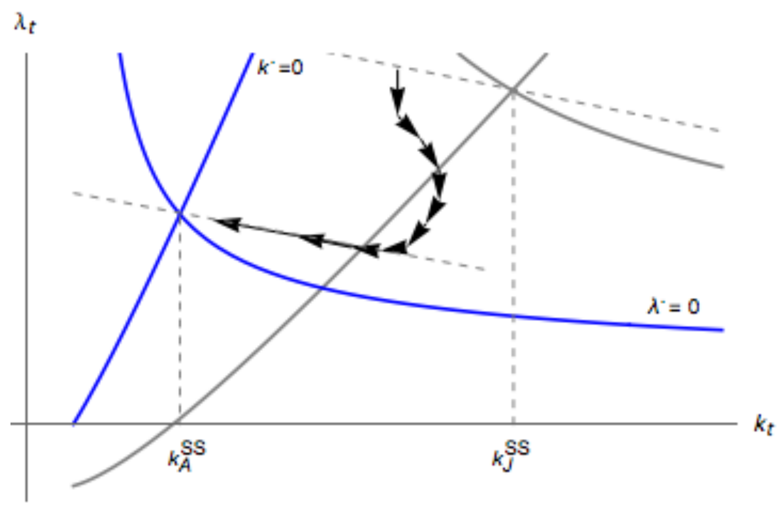
Dynamics Under Anticipated Adult Sanctions

In this section, I describe the optimal response to the anticipation of higher sanctions S_A for $t \geq T$. Graphically, individuals anticipate that both the $\dot{k}_t = 0$ and $\dot{\lambda}_t = 0$ loci will shift to the left for $t \geq T$, as shown in Figure 2. The $\dot{k}_t = 0$ locus shifts up and to the left because the increase in sanctions makes it more expensive to replenish depreciated capital. The $\dot{\lambda}_t = 0$ locus shifts down because c_t is expected to fall in the future (due to higher costs) and this lowers the future return to criminal capital. Figure 2 also shows that the new steady state level of criminal capital k_A^{SS} will be lower than k_J^{SS}.

[33] There is a lower bound k_{min} such that no capital accumulation will take place if $k_0 < k_{min}$ (the asymptote of the $\dot{\lambda}_t = 0$ locus on the k-axis). I focus on individuals for whom $k_{min} < k_0 < k_J^{SS}$ and describe c_t and k_t as they move along the saddle path towards k_J^{SS}.

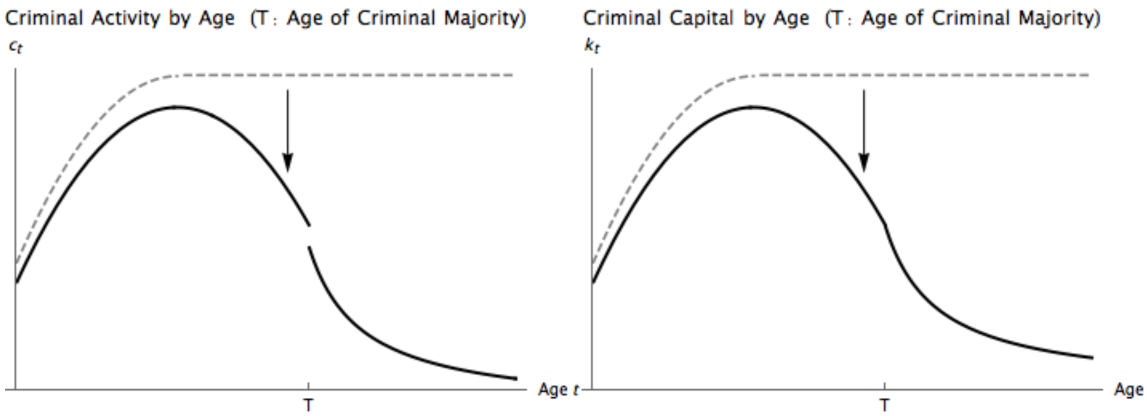
To characterize the optimal response to an anticipated rise in sanctions, we use two pieces of information. First, while the lower sanctions S_J are in effect the original $\dot{k_t}$ and $\dot{\lambda_t}$ functions still dictate the evolution of k_t and λ_t - graphically, the original arrows indicate how $\dot{k_t}$ and $\dot{\lambda_t}$ evolve while $t < T$. Second, the shadow value of criminal capital λ_t cannot jump (decrease discontinuously) at time T, since no new information about sanctions is learned at time T. Instead, λ_t will jump down (decrease discontinuously) when the individual first learns about the higher sanctions S_A. As Figure 2 shows, this ensures that the individual moves toward the new saddle path during $t < T$, and is on the new saddle path at time T. The individual then moves up along the saddle path, decumulating criminal capital until he reaches the new steady state k_A^{SS}.

FIGURE 2: CRIMINAL CAPITAL ACCUMULATION UNDER ANTICIPATED ADULT SANCTIONS



These dynamics dictate how criminal activity and criminal capital evolve as individuals age into adulthood. Figure 2 shows that while individuals are below the ACM T, they will first add to their stock of criminal capital k_t, and later begin to decumulate k_t as they approach T. Since, the change in k_t depends on c_t net of depreciation, this also tells us about the behavior of c_t, which first increases and then decreases as individuals approach T. Optimal c_t drops discontinuously when individuals surpass T and face higher sanctions, and continues to decline as k_t declines (since k_t determines the return to crime). Figure 3 plots the evolution of both k_t and c_t over time. We can see from Figure 3 that deterrence shows up as a discontinuous drop in c_t at T, but deterrence effects also generate lower c_t and k_t prior to reaching the threshold T. This is a deterrence effect because in the absence of adult sanctions, c_t and k_t would have converged towards their original steady state levels (represented by the dotted grey lines).[34]

FIGURE 3: c_t AND k_t UNDER ANTICIPATED ADULT SANCTIONS



Notes: The dashed line marks the optimal paths for c_t and k_t if sanctions stay fixed at S_J.

Comparative Statics

Entry Decisions

In the above analysis, each individual's non-crime utility was normalized to zero. It is straightforward to show that if the outside option (or alternative to crime) improves, individuals are less likely to commit crime in the first place.

Entry decisions are also influenced by the initial stock of criminal capital k_0, since it determines the payoff to crime. Individuals who begin wth a high initial stock of criminal capital, perhaps because they live in areas where returns to crime are high or their peers are criminally active, are predicted to be more likely to begin offending, leading to a self-perpetuating cycle of increases in criminal capital and criminal activity. This prediction is consistent with papers that document very large geographic heterogeneity in criminal offending, including the existence of crime "hot spots" (Eriksson *et al.* 2016, O' Flaherty & Sethi 2014).

[34]It is not necessary that (k_t, λ_t) cross the original $\dot{k_t} = 0$ locus, as shown in Appendix Figures A.3 and A.4. Here, k_t and c_t continue to increase until age T, but are lower than they would be in the absence of adult sanctions. The predicted response to an increase in the age of criminal majority T remains the same.

Myopic Juveniles

Individuals who are not forward looking ($\rho = \infty$) will maximize flow utility, and not lifetime utility. This means that they will not internalize the future benefits of criminal capital while making decisions. The maximization problem is a static one (as in Becker 1968), in which individuals commit crime if the current benefits outweigh the current costs. Therefore, the amount of criminal activity that individuals at age t with criminal capital k_t will undertake is given by

$$c_t = \frac{k_t^\alpha}{2s_t}$$

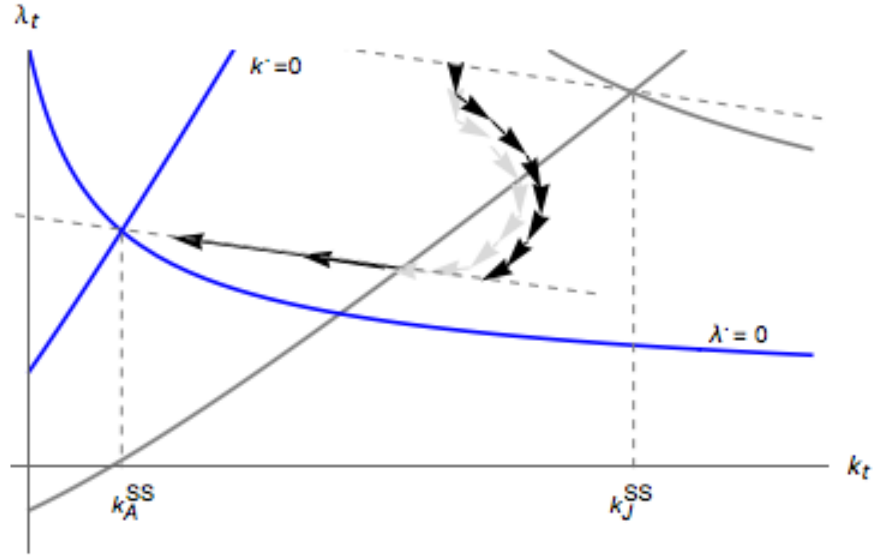
In this case, criminal activity should decrease sharply when sanctions s_t rise as individuals cross the ACM, and the only tests for deterrence are to compare juveniles on either side of the threshold, or examine the behavior of the "newly juvenile group" (the group between T and T') when the age threshold is moved from T to T'. However, past estimates of the change in criminal activity at the threshold have either been small (Lee & McCrary 2017) or negligible (Hjalmarsson 2009, Costa *et al.* 2016). This paper argues that these small effects could be due to mismeasurement of official data, but also because individuals who are deterred by the threat of adult sanctions may exit criminal lifestyles even before reaching the threshold.

Forward-Looking Adolescents

This section focuses on the subset of adolescents who are both informed of the age threshold, and forward looking ($\rho < \infty$). The model predicts that that when the ACM is raised from T to T', three groups should increase criminal activity - age groups close to but below T, age groups between T and T', and age groups close to and above T'. These predictions are tested in Section 5 using recent ACM variation in the Northeastern states.

The first group to benefit from the ACM rise from T to T' is individuals below T', since each of them will face lower sanctions (if caught) for an additional year. In response, individuals will begin to increase criminal activity and accumulate additional criminal capital, as shown in Figures 4 and 5. For instance, a sixteen year old who would have reduced criminal offending and exited his gang before he turned seventeen (T), may postpone exit for an additional year when the ACM is shifted

FIGURE 4: RESPONSE TO AN INCREASE IN T



to eighteen. This will show up as an increase in gang membership and criminal offending by sixteen year olds. Moreover, this response is unlikely to be offset by changes in reporting behavior because sixteen year olds are treated as juveniles both before and after the policy change.

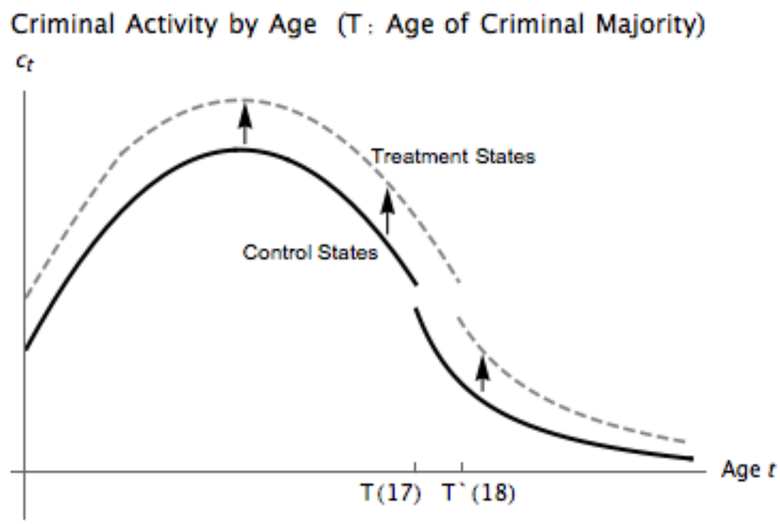
The second set of beneficiaries is the age group between T and T'. These age groups were treated as adults before the policy change, but are treated as juveniles after, and should also increase criminal activity c_t. However, if this policy change is accompanied by a simultaneous increase in under-reporting, we may not observe an increase in official crime statistics for this age group.

Finally, an ACM increase from T to T' will also lead to more criminal activity by age groups above T'. This is because criminal capital (and hence, the return to crime) is higher for age groups aged T' and up. Since these age group are treated as adults both before and after the ACM change, this response is unlikely to be offset by changes in reporting behavior, and we should observe an increase in reported crime.

Suggestive Evidence from the NLSY

To provide suggestive evidence consistent with the predictions of the model, I examine the age profile of self-reported gang membership and criminal involvement using data from the National

FIGURE 5: CRIME RESPONSE TO AN INCREASE IN T



Longitudinal Survey of Youth. A panel of 8,984 adolescents were asked about gang membership and criminal involvement (property, drug, assault and theft offenses) in the twelve months preceding the interview. I use these self-reports to examine whether (1) gang membership and criminal involvement decreases as individuals approach the ACM and (2) whether this decrease begins earlier in states that set the ACM at seventeen instead of eighteen.

The first panel of Figure 7 displays the relationship between gang membership and age for adolescents in all U.S. states that set their ACM at 17 or 18 (as in Pyrooz & Sweeten 2015). Gang membership peaks at ages fifteen and sixteen and declines at ages seventeen and above. The second panel of Figure 7 also shows the age profile of male gang membership, but separates states by their ACM. Here, we find evidence suggestive of earlier exit in states that set the ACM at seventeen, consistent with the predictions of the model. In particular, gang membership peaks earlier (at fifteen) and begins its decline earlier (at sixteen) in states that set the ACM at seventeen. In states that set the ACM at eighteen, gang membership peaks at sixteen, and then declines at ages seventeen and eighteen. Figure A.5 shows that including female respondents leads to similar patterns of gang membership by age.

Figure 8 examines whether the relationship between criminal involvement and age varies with the ACM. The first panel depicts this relationship for adolescents in all U.S. states that set their ACM at 17 or 18 - we see a clear upward trend until sixteen, and a sharp decline at seventeen.

The second panel also displays the age-crime relationship but separates states by their ACM. Two points are worth noting about this graph. One, criminal involvement in higher for all age groups under eighteen. Two, the decline in criminal involvement begins earlier (at age sixteen) in states that set the ACM at seventeen, and appears later, at age seventeen, in states that set the ACM at eighteen. Both patterns are consistent with the predictions of the model. Figure A.5 repeats this analysis for the sample including female respondents and shows that patterns of criminal involvement by age are similar. In Section 5, I show that this pattern of higher criminal involvement for all age groups under eighteen is at least partly driven by the higher age of criminal majority.

4 Empirical Strategy

This section describes the difference-in-difference-in-difference (DDD) framework used to identify the impact of an ACM increase on adolescent offending. I restrict attention to the six neighboring Northeastern states that have introduced legislation to raise the ACM since 2006. These states can be divided into two groups - those in which the legislation was successful and the ACM was modified (Connecticut, Massachusetts, New Hampshire and Rhode Island) and those in which the ACM was left unchanged (New York, Vermont). The DDD technique compares those who were affected by the ACM increase (adolescents) with individuals that were not (older adults) in the two groups of states, before and after the ACM change.

Central Specification

I estimate the following DDD specification with age, state and year fixed effects, as well as age-state, state-year and age-year interactions:

$$C_{alsmy} = \beta_0 + \beta_1 AFFECT_a * TREAT_s * POST_{smy} + \gamma_a + \gamma_s + \gamma_y + \gamma_{as} + \gamma_{sy} + \gamma_{ay} + \gamma_{my} + \varepsilon_{alsmy}$$

C_{alsmy} is a measure of the crime rate among age group a in location l in state s during month m of year y. As a measure of the crime rate, I use the number of arrestees aged a per 100,000 residents in location l. State and age fixed effects (γ_a and γ_s) account for permanent differences across states and age groups. Year fixed effects γ_y account for national crime trends. I also include month fixed effects γ_{my} to control more flexibly for national crime trends.

This specification includes a full set of state-year interactions γ_{sy} which control flexibly for factors that may be changing at the state-year level that could affect my outcomes of interest. Age-state interactions γ_{as} allow for permanent differences across age groups in different states. Age-year interactions γ_{ay} control flexibly for national trends that may affect one age group more than another. Since treatment varies at the age level within each state, standard errors ε_{alsmy} are clustered at the age-state level.

$AFFECT_a$ is an indicator variable that equals one for age groups 21 and under. $TREAT_s$ is an indicator variable that equals one if state s raised its ACM during the study period 2006-15.[35] $POST_{smy}$ is an indicator variable that equals one if the ACM change in state s is in effect in month m of year y. The coefficient of interest is β_1, which is the DDD estimate of the effect of an ACM increase on adolescent offending.

Event Study Specification

In order to examine the year-by-year impact of the ACM change, I use the following event study specification:

$$C_{alsmy} = \sum_{i \geq -n} \beta_i AFFECT_a * TREAT_s * POST^i_{smy} + \gamma_a + \gamma_s + \gamma_y + \gamma_{as} + \gamma_{sy} + \gamma_{ay} + \gamma_{my} + \varepsilon_{alsmy}$$

C_{alsmy} is a measure of the crime rate among age group a as described above. $POST^i_{smy}$ are indicator variables that equal 1 if the ACM was increased in state s exactly i years before period t. For instance, Connecticut raised its ACM from 17 to 18 on July 1, 2012, so the $POST^1$ dummy equals 1 for Connecticut during July 2012 - June 2013, the $POST^2$ dummy equals 1 for Connecticut for the period July 2013 - June 2014, and so on. Also notice that i may take on negative values, which allows us to test for differences prior to the policy's implementation. Regressions continue to control for age, state and year fixed effects, as well as age-state, state-year and age-year interactions.

[35] Rhode Island *lowered* its ACM from 18 to 17 for the period July - November 2007. $TREAT_{s=Rhode\ Island}$ takes on the value -1, which ensures that β_1 can be interpreted as the impact of an increase in the ACM.

Standard errors are clustered at the age-state level to adjust for serial correlation.[36]

Crime Indices

As mentioned above, I use the number of arrests per 100,000 residents as a measure of the local crime rate. However, this outcome variable may be comprised mostly of a handful of frequently occurring offenses such as theft, and not account adequately for serious but less frequent offenses such as homicide. To overcome this drawback of the raw arrest rate, I create a crime index based on offense-specific arrest rates as an alternative measure of local crime. Each index is defined as the equally weighted average of the z-scores of its components (offense-specific arrest rates). Z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation.[37]

I construct two crime indices. The first index uses arrest rates for offenses categories that have a medium or high level of street gang involvement as per the FBI's 2015 National Gang Report. These include drug, homicide, robbery, assault, burglary, theft (including motor vehicle theft and stolen property offenses), forgery and fraud, vandalism and weapon law violations. The second index uses arrest rates for offense categories that have at most a low level of street gang involvement. These include arson, embezzlement, gambling, offenses against the family and children, driving under the influence and liquor laws, disorderly conduct (including drunkenness), and suspicion (including vagrancy and loitering). The objective of this separation exercise is not to suggest that other crimes cannot react to the ACM change - in fact, they may react strongly if there is enough overlap between "gang" and "non-gang" crimes. Instead, the aim is to test whether the types of crime that benefit from the accumulation of criminal capital actually do respond to the ACM change.

[36] Since Rhode Island only changed its ACM for four months before reversing it back, I include it in the control group for the event study regressions.

[37] Results are robust to using scores based on inverse variance weighting and are available on request.

5 Results

In this section, I show that postponing the threat of adult sanctions leads to an increase in juvenile offending. When the age of criminal majority is increased from 17 to 18, individuals aged seventeen and under increase criminal activity. This increase is driven by offenses related to street gangs, including drug, homicide, robbery, theft, vandalism and burglary offenses. A back-of-the-envelope calculation shows that for every seventeen year old that was transferred to juvenile facilities as a result of the ACM increase, jurisdictions bore social costs of $65,000 due to the increase in juvenile offending.

The setting for the empirical tests is a group of neighboring Northeastern States, namely Connecticut, Massachusetts, New Hampshire, New York, Rhode Island and Vermont. Each of these states has experimented with raising the ACM since 2005, lending credibility to the assumption that the actual timing of ACM changes within these states was exogenous, or unrelated to local crime trends. These results are based on a balanced panel of agencies that submit data via the Uniform Crime Reports for the period 2006-15.

5.1 Juvenile Crime

I first test whether increasing the ACM from 17 to 18 led to an increase in overall arrest rates for 13-17-year-olds. These results are presented in the first column of Table 1, which shows that the monthly arrest rate increased by around 0.31, or 6 per cent of the mean, for each age group in the range 13-17.

The second column reports analogous estimates for arrest rates for offenses with a medium or high level of street gang involvement, based off of the FBI's 2015 National Gang Report. Here we see that the previously documented increase is entirely driven by offenses associated with street gangs, for which arrest rates increase by 0.34, or 7 per cent of the mean. Since the above estimate may be driven by a handful of frequently occurring offenses, I also examine effects on a crime index based on gang-related offenses (which weights offense categories equally). The lower panel of Table 1 shows that the gang-related crime index increases, and that this estimate is statistically significant at the 1 per cent level.

The third column reports analogous estimates for offenses with at most a low level of street gang involvement, such as driving under the influence and liquor law violations. These offense categories do not respond to the increase in the ACM - the estimated coefficient is small and statistically indistinguishable from zero. Using a crime index based on these offense categories leads to similar results - the estimated coefficient is negative and statistically indistinguishable from zero.

5.2 Age-Specific Estimates

The previous section presented a broad overview of the average impact of the ACM increase on juvenile crime. In this section, I present age-specific estimates of the impact of the ACM increase on both gang-related and other offenses. As predicted by the model, I show that the ACM increase also leads to an increase in offending by those above the age of eighteen, since these adolescents are now associated with a higher level of criminal capital, and their return to offending will be higher than previous cohorts'.

Table 2 displays the estimated impact on arrest rates and crime indices for each age group in the age range 13-21.[38] The first two columns present results for offenses with a medium or high level of street gang involvement - arrest rates increase for 13-16-year-olds by around 12 per cent of the mean, and for 18-year-olds by around 8 per cent of the mean. The first panel of Figure 9 displays these age-specific estimates graphically. The smaller, statistically insignificant estimate for 17-year-olds is particularly conspicuous. However, this finding may be driven by the fact that seventeen year-olds are simultaneously exposed to lower sanctions as well as an increase in underreporting. The latter effect appears to offset the effect of lower sanctions, which can be observed much more clearly for those aged 13-16 and 18.

Next, I use an event study specification to examine the year-by-year impact of the ACM increase on arrests for gang-related offenses in the age group 13-21. The second panel of Figure 9 displays DDD coefficients for five years before and four years after the policy's implementation. We see a clear increase in arrest rates starting in the first year of the ACM change. Further, this effect increases over time.

[38] The Uniform Crime Reports only report collective data for 13 and 14 year old arrestees.

I also use the gang-related crime index as an alternative outcome variable to confirm that the previous results are not driven by a a handful of frequently occurring offenses. The second column in Table 2 shows that the ACM increase leads to a statistically significant increase in offending by all age groups aged 13-21. Figure 10 displays these age-specific estimates graphically, showing that the largest increases are observed for younger age groups. The second panel of Figure 10 displays DDD coefficients for five years before and four years after the policy's implementation, showing a clear break in the first year of the policy's implementation and an increasing coefficient over time.

Finally, I examine effects on crime categories that are not commonly associated with street gang involvement. The last two columns of Table 2 show that neither the arrest rate nor the crime index increase significantly for any of the age groups. This indicates that general crime trends are unlikely to be driving the reported effects on gang-related crime. The last panel of Figure 11 displays year-by-year estimates from an event study specification to show that the increase in the gang-related crime index is not mirrored by a similar increase in the crime index based on other crime categories, following the ACM increase. Separate estimates for 17-year-olds show that the estimated effect is actually negative during the first year of the policy's implementation. This is in line with a surge in under-reporting of 17-year old offenders, who are now treated as juveniles by the criminal justice system.

5.3 Offense-Specific Estimates

In this section, I present results for arrest rates by offense category. This includes ten offense categories associated with street gang activity, and nine offense categories that are not.

Table 3 displays DDD estimates of the increase in arrests for offenses with a medium or high level of street gang involvement. These estimates are displayed separately for 13-16-year-olds, 17-year-olds and 18-21-year-olds. I find that the arrest rate for 13-16-year-olds increases by over 15 per cent of the mean for drug offenses and by over 20 per cent of the mean for homicide, robbery, theft, burglary and vandalism offenses. This increase is not observed for 17-year-olds, and is only observed for a subset of offenses for 18-21 year olds. In sum, the evidence points to a consistent increase across gang-related crime categories for 13-16 year olds, but a less consistent increase for other age groups close the ACM.

To show that general crime trends may not explain the effects documented above, I also examine the effect of the ACM increase on crime categories that are less likely to involve street gangs. Table 4 displays DDD estimates for nine offense categories, separately for 13-16-year-olds, 17-year-olds and 18-21-year-olds. There appears to be no consistent response for these offense categories amongst 13-16 and 17-year-olds, since many of the estimates are negative, and most are statistically indistinguishable from zero. There is a statistically significant increase in the 18-21-year-old arrest rate for disorderly conduct and liquor law violations. However, the estimated impact of the ACM increase on all other offense categories is small (relative to the mean) and statistically indistinguishable from zero.

5.4 Demographic Heterogeneity

In this section, I examine which gender and race groups are driving the increase in gang-related juvenile crime. The first panel of Table 5 shows a significant increase in arrest rates for males of each age except 17, while the second panel shows a less statistically significant pattern for females. This finding may be due to the low involvement of females in criminal enterprises like gangs - for instance, the 2011 National Youth Gang Survey reports that the proportion of female gang members did not exceed 8 per cent over the period 1998-2010.

The UCR data also report the number of arrests of individuals aged seventeen and under by race. The last panel of Table 5 shows that the deterrence estimates are largely driven by an increase in the arrest rate for White adolescents[39] while the response among Black and Asian adolescents is statistically indistinguishable from zero. This is an intriguing finding because the National Youth Gang Survey reports that in 2011 around 58 per cent of gang members were White/Hispanic while 35 per cent were Black. However, this pattern is consistent with effective treatment differing across race groups. If youth of color are disproportionately charged in adult courts, as reported in Juszkiewicz (2009), raising the ACM may change their incentives less than those of White youth. In this situation, it would not be surprising to find larger effects for White adolescents and smaller

[39] Agencies do not separately report arrests for Hispanic arrestees, who can be included in any of the race categories.

effects for adolescents belonging to other race groups.

5.5 Robustness Checks

This section shows that the main results are robust to a number of checks such as varying the level of clustering, using alternative age cohorts as control groups and extending the study sample to all states in the U.S. The outcomes of interest are the 13-17-year-old arrest rate and crime index based on gang-related offenses.

Clustering at the Juvenile-State Level

Clustering standard errors at the age-state level allows for serial correlation within each age group, but not across juvenile age groups within states. I relax this assumption by clustering at the juvenile-state level, which allows errors to be serially correlated across all juveniles within a given state, and across all adults within a given state.

Table A.3 shows that clustering standard errors at the juvenile-state level does not materially change the main results. The first column shows that while the confidence interval is wider, the estimated increase in the arrest rate remain statistically significant at the 5 per cent level. Table A.4 reports analogous results for the juvenile gang crime index. Once again, the confidence interval widens, but the estimated increase remains significant at the 5 per cent level.

Alternative Age Groups as Controls

The previous analysis defines individuals aged 22 and above as control groups. The second and third columns of Table A.3 shows that the estimated increase in juvenile arrests is not driven by this choice, and is robust to redefining the control group to include only younger or older age groups. Table A.4 reports analogous results for the juvenile gang crime index. Both sets of results are robust to clustering at the juvenile-state level.

Geographical Spillovers

One possible mechanism behind the increase in juvenile crime is that crime is simply spilling over from treatment to control states, with no increase in overall crime. I attempt to shed light on this hypothesis by dropping jurisdictions that border treatment and control states.[40] The fourth column of Table A.3 shows that this exercise does not alter the estimated increase in the 13-17-year-old arrest rate, suggesting that geographic spillovers are not driving the above findings. Table A.4 reports analogous results for the juvenile gang crime index. Both sets of results are robust to clustering at the juvenile-state level.

Other Juvenile Justice Reforms

A natural worry with studies that exploit ACM changes is that they are likely to have been accompanied by other juvenile justice reforms. This worry is partly assuaged by the fact that I control for state-year shocks, which would pick up the effect of justice policy reforms that affect all adolescents uniformly. I also show evidence for a reaction to the ACM by 18-year-olds, who are treated as adults both before and after the ACM change, and would be unaffected by other reforms that explicitly target juveniles.

Amongst the treatment states, Connecticut implemented a range of juvenile justice reforms such as reducing in-school arrests in 2009 and discontinuing the detention of juveniles for non-criminal cases in 2007. These reforms were not implemented in the same year as the ACM increase, and the event study estimates indicate that these policies did not have a large impact prior to the ACM change. Moreover, these policies serve to reduce the number of juvenile arrests, which would lead to an underestimate of the true effect of the ACM increase. To show this formally, I restrict attention to the three treatment states that implemented the ACM change without an accompanying package of reforms, as well as the two control states New York and Vermont.

The fifth column of Table A.3 displays the estimated increase in the juvenile arrest rate after excluding Connecticut from the study sample. The estimated increase is larger, consistent with the fact that accompanying reforms would reduce juvenile arrests. Table A.4 reports analogous results

[40] Table A.2 displays the list of police agencies that are dropped from the analysis.

for the juvenile gang crime index. Both sets of results are robust to clustering at the juvenile-state level.

Including Additional States

As a final check, I show that the main results are robust to widening the sample to include additional states. The sixth column of Table A.3 extends the sample to include all states in the Northeast.[41] The last column extends the sample to include all states in the U.S.[42] Both columns show a statistically significant increase in the juvenile gang-related arrest rate. The last two columns of Table A.4 report analogous results for the juvenile gang crime index. These results do not change meaningfully when we cluster at the juvenile-state level.

5.6 Some Costs of Raising the Age of Criminal Majority

This section uses a back-of-the-envelope calculation to compare the social costs of raising the ACM with its expected benefits. This is necessarily a partial estimation exercise, since I make multiple assumptions and focus only on two sources of social cost increases due to the ACM change - the increase in criminal offending by 13-16-year-olds and the costs of transferring 17-year-olds to more expensive juvenile facilities. These cost estimates are then compared with the expected benefits of raising the ACM, which include higher earnings for 17-year-olds without criminal records. While the expected benefits of raising the ACM appear to offset the expected costs, an important takeaway from this exercise is that these costs exist and can be sizable, contrary to the findings of previous studies.

The first source of social costs due to the ACM change is the increase in criminal offending by age groups below seventeen. For each crime category, I use the arrest-to-offense ratio from the 2015 UCR data to predict the increase in the number of offenses by 13-16-year-olds.[43] The first two columns of Table 6 displays the estimates of the increase in monthly arrest rates of 13-

[41] These include three additional states - New Jersey, Pennsylvania and Maine.

[42] This sample includes two additional treatment states - Illinois (post 2009) and Mississippi (post June 2011).

[43] This ratio does not include offenses that are not reported to the police and is therefore an underestimate of the total increase in offending. This method will also underestimate the increase in offending if juveniles are arrested at lower rates than adults.

16-year-olds for homicide, assault, robbery, larceny, motor vehicle theft, stolen property, burglary, vandalism, fraud, forgery and drug offenses following the ACM increase as well as the arrest-to-offense ratio for each of these crimes. The third column displays McCollister *et al.* (2010)'s estimates of societal costs by offense type, which include costs imposed directly on victims and indirectly on the criminal justice system in the form of legal, police and corrections costs.[44] The fourth column displays the annual increase in costs (including incarceration[45]) by offense type due to the uptick in offending, evaluated at the average treatment agency population of 27,200. Overall, the crime increase among 13-16-year-olds led to an increase of $265,000 in societal costs, two thirds of which is accounted for by homicide offenses.

The second source of social costs is the transfer of 17-year-olds to juvenile facilities. Juvenile incarceration costs in the treatment states average $544 per day (Justice Policy Institute 2014), while the equivalent estimate for adult incarceration is $198 (Vera Institute of Justice 2017).[46] My estimate of the number of such transfers is based on the Office of Juvenile Justice and Delinquency Prevention's data on offense-specific probation and incarceration rates for 17-year-olds, displayed in Table 7.[47] The sixth column displays completed sentence lengths specific to each offense category (person, property, drug and public order) also reported by the OJJDP. The increase in costs due to the transfer of 17-year-olds from adult to juvenile facilities is just under $75,000.[48]

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[44] McCollister *et al.* (2010) employ cost-of-illness and jury compensation methods to estimate both the tangible and intangible costs of crime. I use their estimates for three reasons - first, they provide the most recent set of estimates; second, they provide cost estimates for more offense categories than Donohue III (2009); third, their estimates for the overlapping set of offenses are broadly similar to those of other studies like Donohue III (2009) and Cohen *et al.* (2004). I exclude McCollister *et al.* (2010)'s estimates of offenders' productivity losses while incarcerated, since individuals below seventeen are unlikely to be a part of the formal labor force. I also supplement these estimated with Mueller-Smith (2016)'s social cost of drug offenses estimate of $2,544. I exclude simple assault and weapon law violations due to the lack of social cost estimates for these offenses.

[45] This is likely to be an underestimate, since juvenile incarceration costs over twice as much as adult incarceration. I also do not account for the fact that 13-16-year-old offenders who are incarcerated may be more likely to recidivate in the future.

[46] Since New Hampshire is not included in the Vera Institute of Justice 2017 report, I use the Vera Institute of Justice 2012 estimates assuming that its costs grew at the same rate as Connecticut and Rhode Island, who provided information in both surveys. Estimates are in 2015 USD.

[47] Here, I make three assumptions. One, the proportion of 17-year-olds adjudicated delinquent that receive placement sentences (instead of probation sentences) does not change after the increase in the ACM. Two, the cost of probation for a 17-year-old does not change when the ACM is raised to eighteen. Third, the completed duration of incarceration does not depend on the ACM, an assumption supported by the findings of Fritsch *et al.* (1996) and Fagan (Jan/Apr. 1996.).

[48] If the marginal incarceration cost is around half of the average cost in both juvenile and adult facilities (as found by Owens (2009) in Maryland) the cost increase will be around $37,500.

What are the benefits of raising the ACM? Proponents of raising the ACM argue that juvenile justice policies reduce recidivism. However, recent studies show that incarceration in juvenile facilities has a large impact on recidivism (Aizer & Doyle 2015) and that adult incarceration may actually lower recidivism for marginal offenders (Loeffler & Grunwald 2015a). Therefore, I do not focus on lower recidivism as the primary benefit of raising the age; instead, I estimate the number of 17-year-olds who will be diverted away from adult prisons and will not receive criminal records. This estimate is displayed by offense type in the fourth column of Table 7, which sums up to a total of 5.35 17-year-olds.

The question for policymakers is whether a cost increase of $65,000 per 17-year-old is exceeded by the potential benefits. There are three reasons why it might - one, the expungement of criminal records has been shown to increase college completion rates (Litwok 2014), boost employment and average annual real earnings by around $6,000 (Chapin *et al.* 2014, Selbin *et al.* 2017) and reduce government dependence and increase tax revenues by around $2,200 (Chapin *et al.* 2014); two, the transfer to juvenile facilities may lower the risk of assault faced by the average juvenile convict - McCollister *et al.* (2010) estimate victim costs alone of over $200,000 for sexual assault and $100,000 for aggravated assault;[49] three, if more 17-year-olds receive probation instead of incarceration sentences, Aizer & Doyle (2015) and Bayer *et al.* (2009)'s findings indicate that we may see an increase in high school completion rates and a decrease in recidivism. Focusing on the increase of $6,000 in earnings alone indicates that increasing the ACM may have been a move in the right direction.

6 Conclusion

Recent research shows that criminal involvement can persist into long-term offending, as individuals accumulate skills and experience pertinent to the crime sector (Bayer *et al.* 2009, Pyrooz *et al.* 2013, Carvalho & Soares 2016, Sviatschi 2017) or lose human capital valued in the non-crime sector (Hjalmarsson 2008, Aizer & Doyle 2015). However, existing research on the deterrent effects of the

[49] It is difficult to quantify the change in assault risk faced by adolescents across different types of facilities. For instance, Beck & Hughes (2004) document that rates of reported sexual assault are six times higher in juvenile correctional facilities than in adult facilities across the U.S. This is likely driven by state laws specifying that all sexual acts involving youth below certain ages are nonconsensual.

age of criminal majority largely overlooks these inter-temporal complementarities in the returns to crime.[50]

In this paper, I show that accounting for these dynamic incentives can change how we look for and measure deterrence. This approach also helps us deal with the issue of increased under-reporting as individuals cross the age of criminal majority, which may have biased existing studies towards finding effects of no deterrence. Using policy variation in the Northeastern states since 2006, I find that raising the age of criminal majority increases overall arrest rates for 13-17-year-olds. This rise in arrests is driven by offenses commonly associated with street gangs, including both property and violent offenses. Using a back-of-the-envelope calculation, I show that for every 17-year-old that was diverted from adult sanctions, jurisdictions bore costs of $65,000 due to this increase in juvenile offending. Policymakers deciding where to set the age of criminal majority must acknowledge that these costs can be sizable, and evaluate whether they are outweighed by the potential benefits, such as an increase in educational attainment and employment associated with fewer 17-year-olds having criminal records. This conclusion is particularly relevant today, because states like Connecticut, Illinois and Vermont have introduced legislation to move the age of criminal majority even further to twenty one.

Incorporating dynamic incentives into models of criminal decision making appears to be a rich area for future work (McCrary 2010). While this paper applies this approach to study the deterrent effects of criminal sanctions, it may also be applied to understand the effects of other features of the criminal justice system. For instance, if criminal capital is slow to depreciate, the dynamic approach would indicate that the positive effects of rehabilitative services are likely to be much larger when evaluated over the long term.

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[50] Munyo (2015) is an exception.

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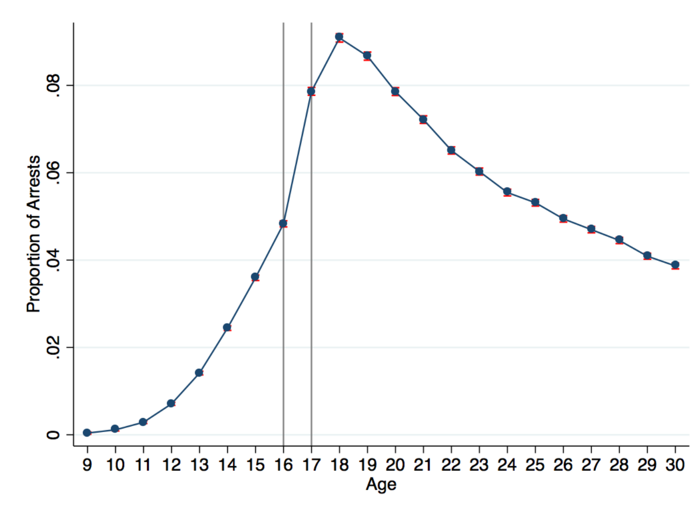
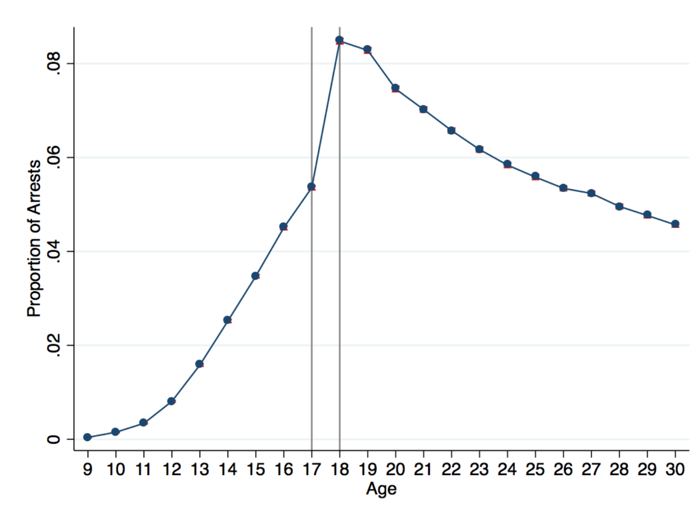
Figures

FIGURE 6: CRIME REPORTING INCREASES AT AGE OF CRIMINAL MAJORITY PROPORTION OF ARRESTS BY AGE 2006-14

(A) AGE OF CRIMINAL MAJORITY = 17



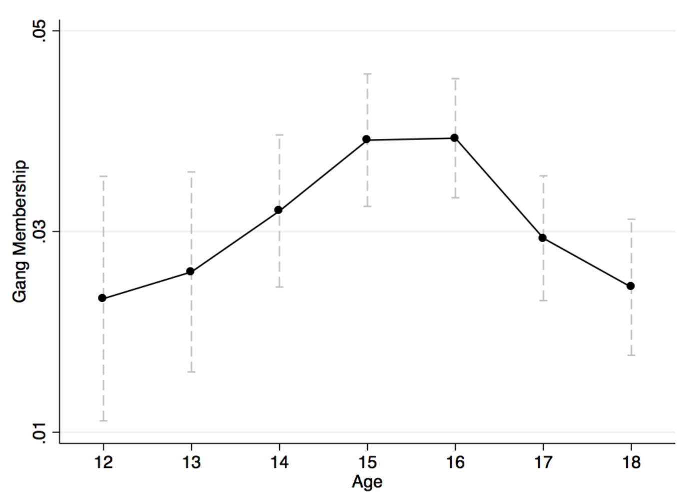
(B) AGE OF CRIMINAL MAJORITY = 18



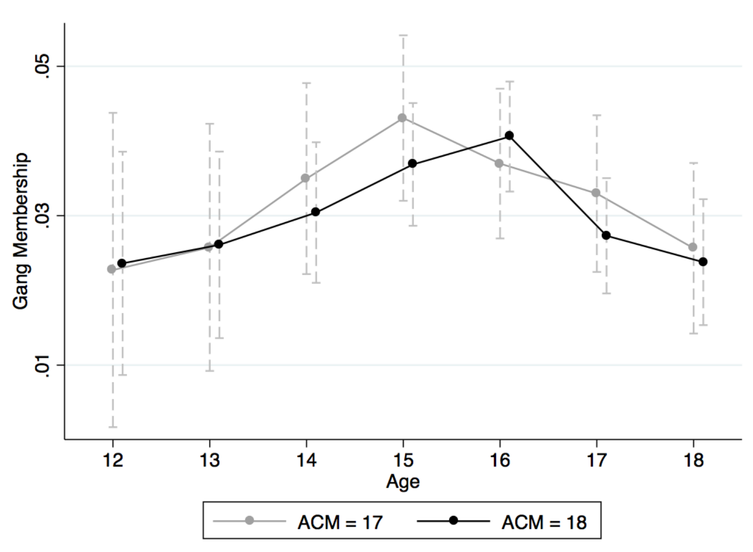
Notes: Uses monthly NIBRS data at the agency level from 39 states. Confidence intervals in red.

FIGURE 7: GANG MEMBERSHIP-AGE PROFILE

(A) ALL STATES



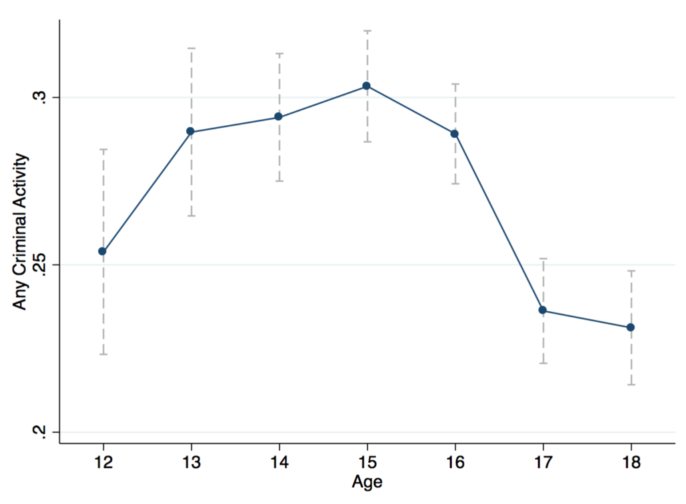
(B) GANG MEMBERSHIP-AGE PROFILE BY AGE OF CRIMINAL MAJORITY



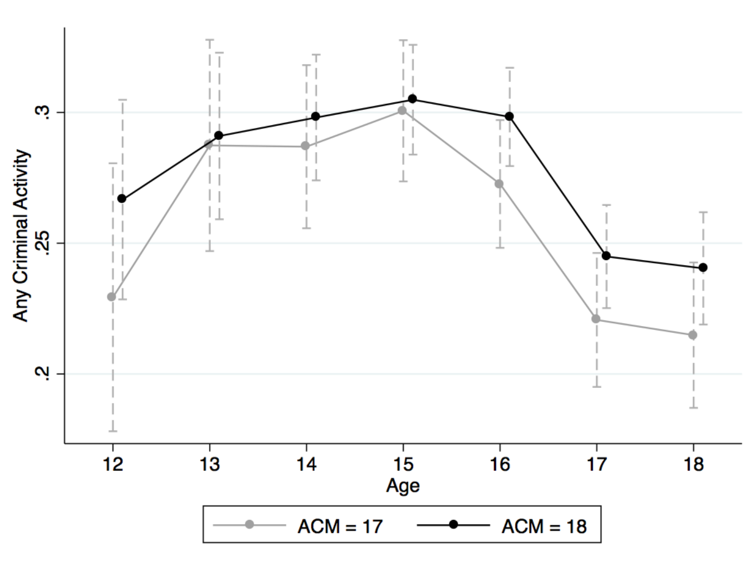
Notes: This graph uses the NLSY97 self-reported data on gang membership by male adolescents. The coefficients are estimates from a regression of gang membership on age-fixed effects.

FIGURE 8: CRIMINAL INVOLVEMENT-AGE PROFILE

(A) ALL STATES



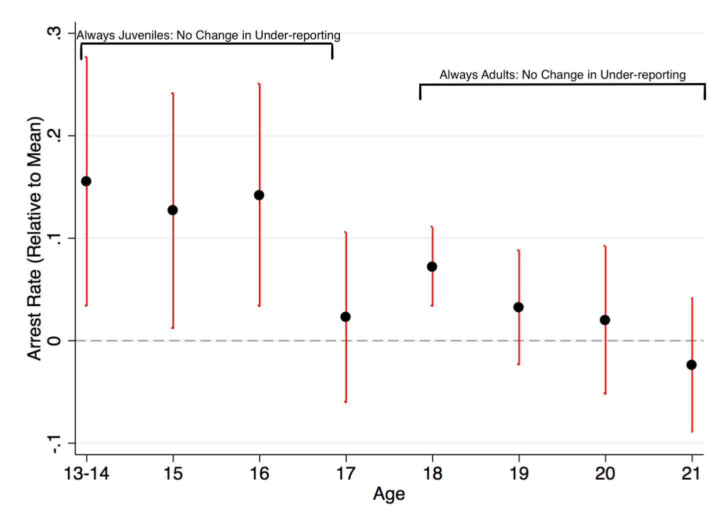
(B) CRIMINAL INVOLVEMENT-AGE PROFILE BY AGE OF CRIMINAL MAJORITY



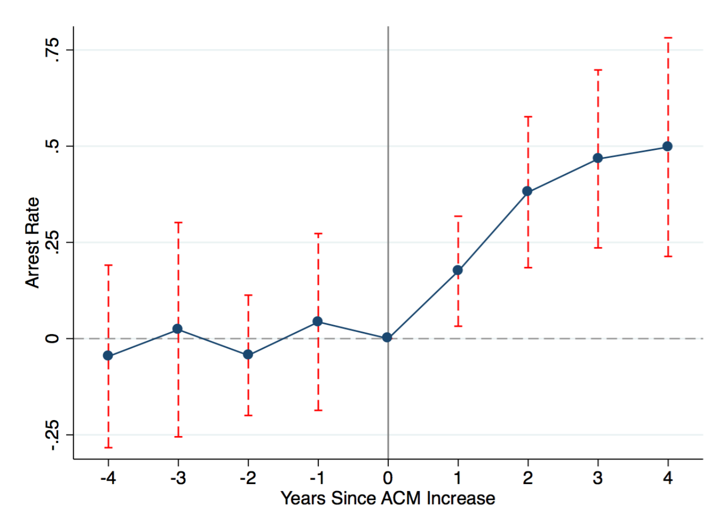
Notes: This graph uses the NLSY97 self-reported data on criminal involvement by male adolescents. The coefficients are estimates from a regression of criminal involvement on age-fixed effects.

Figure 9: Impact of an Increase in the Age of Criminal Majority on Offenses Related to Street Gangs

(a) Age Specific Estimates



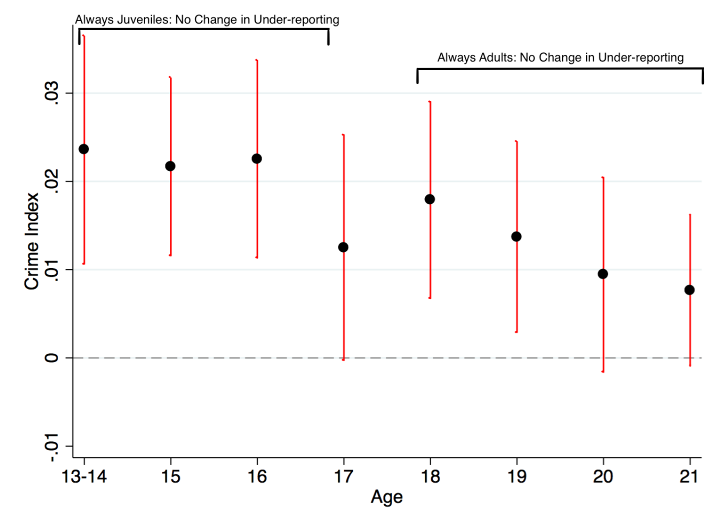
(b) 21 and Under Arrest Rate



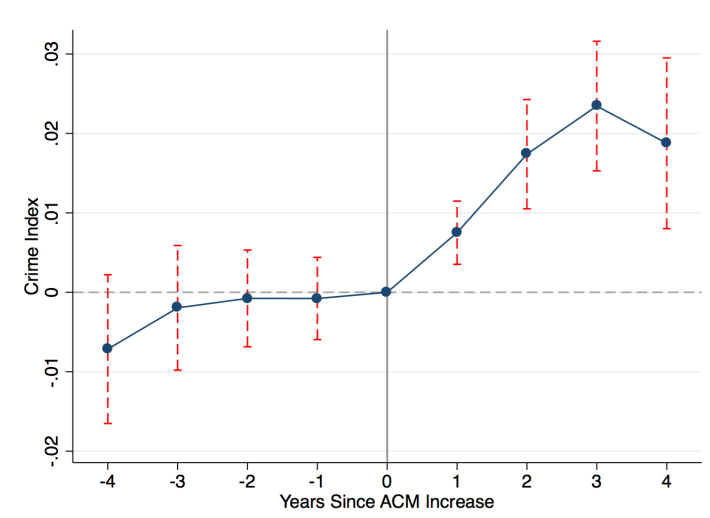
Notes: Figures display the year-by-year estimates (and 95% confidence intervalss) of the impact of an increase in the Age of Criminal Majority from 17 to 18. This figure uses the ACM implementation dates of July 2012 for Connecticut, September 2013 for Massachusetts, and July 2015 for New Hampshire.

FIGURE 10: IMPACT OF AN INCREASE IN THE AGE OF CRIMINAL MAJORITY ON OFFENSES RELATED TO STREET GANGS (CRIME INDEX)

(A) AGE SPECIFIC ESTIMATES



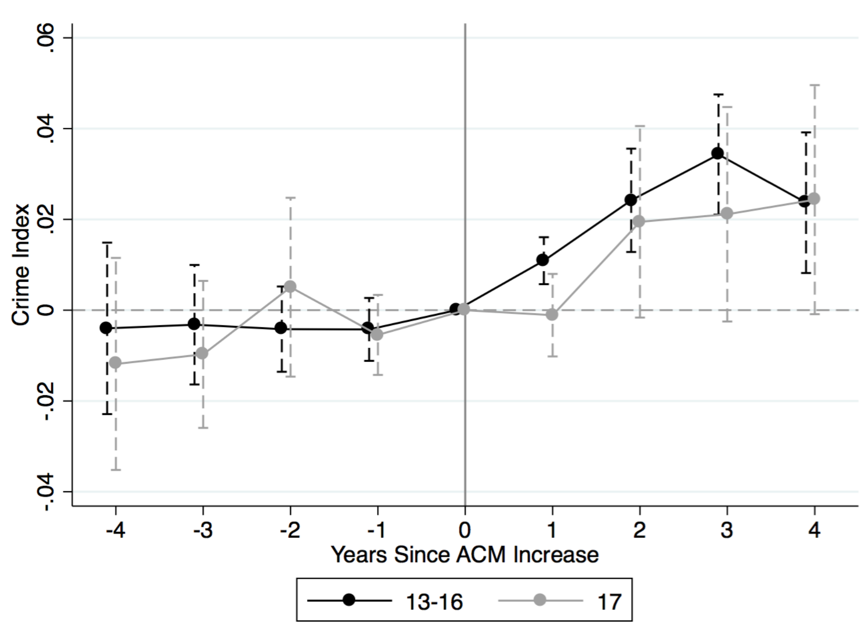
(B) 21 AND UNDER (CRIME INDEX)



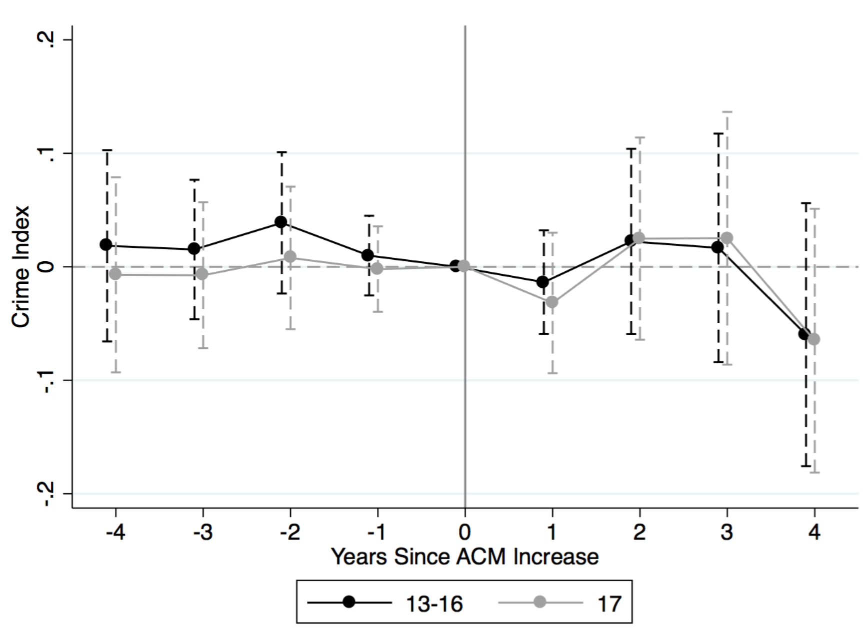
Notes: Figures display the year-by-year estimates (and 95% confidence intervalss) of the impact of an increase in the Age of Criminal Majority from 17 to 18. This figure uses the ACM implementation dates of July 2012 for Connecticut, September 2013 for Massachusetts, and July 2015 for New Hampshire.

FIGURE 11: IMPACT OF AN INCREASE IN THE AGE OF CRIMINAL MAJORITY 17-YEAR OLDS VERSUS 13-16 YEAR OLDS

(A) GANG-RELATED CRIME INDEX



(B) OTHER CRIME INDEX



Notes: Figures display the year-by-year estimates (and 95% confidence intervalss) of the impact of an increase in the Age of Criminal Majority from 17 to 18. This figure uses the ACM implementation dates of July 2012 for Connecticut, September 2013 for Massachusetts, and July 2015 for New Hampshire.

Tables

Table 1: Impact of an Increase in the Age of Criminal Majority on Juveniles Aged 13-17

Type of Offense	All	Gang-Related	Other
	Arrest Rate		
DDD Estimate	0.310**	0.337***	-0.026
	(0.135)	(0.109)	(0.118)
Mean	5.438	3.925	1.301
	Crime Index		
DDD Estimate	0.003	0.019***	-0.013
	(0.015)	(.003)	(0.030)
Mean	0.004	-0.028	0.037
Observations	945,000	945,000	945,000

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Each regression controls for state, year and age fixed effects, as well as state-year, age-year and age-state fixed effects. Standard errors are clustered at the age-state level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2: IMPACT BY AGE GROUP

Age Group	Gang-Related Offenses		Other Offenses	
	Arrest Rate	Index	Arrest Rate	Index
13-14	0.242**	0.024***	-0.183*	-0.015
	(0.101)	(0.007)	(0.103)	(0.044)
Mean	1.695	-0.0240	0.346	0.0170
15	0.360**	0.022***	-0.219	-0.026
	(0.173)	(0.005)	(0.181)	(0.041)
Mean	3.020	-0.0170	0.728	0.105
16	0.652**	0.023***	0.032	-0.010
	(0.253)	(0.006)	(0.206)	(0.045)
Mean	4.579	-0.0380	1.520	0.0120
17	0.160	0.012*	0.255	-0.007
	(0.270)	(0.007)	(0.377)	(0.046)
Mean	6.327	-0.0330	2.612	0.0130
18	0.538***	0.018***	0.796	0.083
	(0.151)	(0.006)	(0.605)	(0.152)
Mean	6.887	-0.0330	4.020	0.413
19	0.246	0.014**	0.999	-0.009
	(0.193)	(0.006)	(0.617)	(0.060)
Mean	6.520	-0.0360	4.188	0.311
20	0.163	0.009*	0.909	0.015
	(0.232)	(0.006)	(0.574)	(0.045)
Mean	5.803	-0.0380	3.911	0.209
21	-0.104	0.008*	0.432**	-0.074
	(0.187)	(0.004)	(0.164)	(0.305)
Mean	5.505	-0.0410	2.850	1.094
Observations	756,000	756,000	756,000	756,000

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Each regression controls for state, year and age fixed effects, as well as state-year, age-year and age-state fixed effects. Standard errors are clustered at the age-state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 3: IMPACT BY OFFENSE TYPE
OFFENSES RELATED TO STREET GANGS

Offense Category	**13-16**	**17**	**18-21**
Drug Crimes	0.077***	0.026	0.014
	(0.029)	(0.085)	(0.070)
Mean	0.498	1.692	2.047
Homicide	0.002**	-0.000	0.002
	(0.001)	(0.001)	(0.001)
Mean	0.001	0.002	0.005
Assault	-0.038	-0.029	-0.011
	(0.052)	(0.077)	(0.025)
Mean	0.957	1.483	1.385
Robbery	0.011**	0.008	-0.017**
	(0.005)	(0.013)	(0.007)
Mean	0.0510	0.100	0.0910
Theft	0.233***	0.071	0.059
	(0.055)	(0.114)	(0.050)
Mean	0.898	1.860	1.594
Stolen Property Offenses	0.050***	0.128***	0.065***
	(0.012)	(0.019)	(0.011)
Mean	0.0900	0.198	0.179
Burglary	0.038***	0.043	0.041**
	(0.013)	(0.035)	(0.019)
Mean	0.199	0.362	0.316
Vandalism	0.113***	0.062	0.075***
	(0.018)	(0.054)	(0.020)
Mean	0.411	0.671	0.473
Weapon Law Violations	0.001	-0.022**	-0.001
	(0.005)	(0.011)	(0.006)
Mean	0.0560	0.0850	0.0900
Fraud & Forgery	-0.028**	0.014	0.059***
	(0.011)	(0.012)	(0.019)
Mean	0.0320	0.105	0.255
Observations	882,000	756,000	945,000

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Each regression controls for state, year and age fixed effects, as well as state-year, age-year and age-state fixed effects. Standard errors are clustered at the age-state level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4: IMPACT BY OFFENSE TYPE
OTHER OFFENSES

Offense Category	**13-16**	**17**	**18-21**
Arson	-0.006	-0.001	0.002
	(0.004)	(0.003)	(0.002)
Mean	0.0210	0.0170	0.0100
Embezzlement	0.002	0.0005	0.002
	(0.001)	(0.003)	(0.003)
Mean	0.003	0.007	0.002
Offenses against the Family & Children	-0.004	-0.031	-0.003
	(0.009)	(0.024)	(0.005)
Mean	0.0320	0.0380	0.0450
Driving Under the Influence	-0.124***	-0.069	0.066
	(0.025)	(0.052)	(0.042)
Mean	0.0180	0.216	0.837
Liquor Laws	0.059	0.216	0.581**
	(0.057)	(0.235)	(0.235)
Mean	0.298	1.352	1.784
Drunkenness	0.028	0.096	0.005
	(0.030)	(0.159)	(0.050)
Mean	0.0760	0.288	0.383
Disorderly Conduct	-0.063	0.050	0.097**
	(0.047)	(0.059)	(0.041)
Mean	0.409	0.679	0.654
Gambling	-0.0003	-0.0001	0.0003
	(0.0003)	(0.001)	(0.001)
Mean	0.0004	0.002	0.002
Vagrancy, Suspicion, Curfew, Loitering	-0.008***	-0.007**	0.001
	(0.002)	(0.003)	(0.002)
Mean	0.00800	0.0150	0.0120
Observations	882,000	756,000	945,000

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Each regression controls for state, year and age fixed effects, as well as state-year, age-year and age-state fixed effects. Standard errors are clustered at the age-state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Impact of ACM Increase on Gang-Related Crime Index by Demographic Group

Gender & Age	**Male 13-14**	**Male 15**	**Male 16**	**Male 17**
DDD Estimate	0.142**	0.249***	0.531**	0.100
	(0.062)	(0.082)	(0.221)	(0.247)
Mean	1.183	2.126	3.254	4.548
Gender & Age	**Female 13-14**	**Female 15**	**Female 16**	**Female 17**
DDD Estimate	0.100**	0.111	0.121*	0.060
	(0.043)	(0.112)	(0.063)	(0.090)
Mean	0.511	0.894	1.325	1.778
Race & Age	**White 0-17**	**Black 0-17**	**Indian 0-17**	**Asian 0-17**
DDD Estimate	1.366***	-0.051	0.186*	0.100
	(0.432)	(0.383)	(0.094)	(0.095)
Mean	14.67	3.177	0.0370	0.135
Observations	756,000	756,000	756,000	756,000

Notes: The UCR data does not contain age specific arrests by race, only the number of arrests under 18 separated by race; Hispanic arrestees are not reported separately and may belong to any of the race categories. Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Each regression controls for state, year and age fixed effects, as well as state-year, age-year and age-state fixed effects. Standard errors are clustered at the age-state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Social Costs of Increase in Juvenile Offending

Offense	**Arrest Rate Increase 13-16-Year-Olds**	**Arrest-Offense Ratio**	**Unit Cost* 2015 $**	**Estimated Cost 2015 $**
Homicide	0.005	61.5	9,717,787	255,060
Robbery	0.038	29.3	41,842	17,689
Aggravated Assault	-0.055	54.0	115,383	-38,193
Burglary	0.163	12.9	6,359	26,323
MV Theft	0.035	13.1	11,241	9,709
Larceny	0.451	21.9	3,706	24,929
Stolen Property	0.159	19.4	7,526	20,112
Vandalism	0.365	19.4	4,575	28,084
Forgery	0.009	19.4	5,066	741
Fraud	-0.011	19.4	4,809	-892
Drug Crimes	0.069	20.0	2,544	5,767
Total				**$ 264,953*** **

Notes: Arrest Rate Increase estimates are based on DDD regressions that estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Cost estimates are evaluated at a population of 27221, the mean for treatment agencies. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 7: SEVENTEEN YEAR OLDS: CHANGE IN ANNUAL COST OF INCARCERATION

Offense	Monthly Arrest Rate	Adjudicated Delinquent (Per 1000)	Waived to Adult Court (Per 1000)	Adjudicated Delinquent (Number)	Incarce-rations (Per 1000)	Duration (Months)	Cost Adult Facilities	Cost Juvenile Facilities
Homicide	0.001	408	65	0.0014	171	8.18	29	80
Rape	0.02	408	65	0.0285	171	8.18	578	1592
Robbery	0.058	459	84	0.0949	227	8.18	2279	6273
Aggravated Assault	0.141	401	36	0.1916	143	8.18	3319	9135
Burglary	0.157	405	29	0.2139	152	5.72	2728	7510
Larceny-theft	1.234	219	9	0.8908	44	5.72	6082	16740
Motor vehicle theft	0.046	388	17	0.0593	172	5.72	894	2460
Other Assaults	0.901	243	9	0.7217	65	8.18	9378	25812
Arson	0.008	330	4	0.0087	77	5.72	68	187
Forgery, Counterfeiting	0.013	272	14	0.0117	77	5.72	112	309
Fraud	0.012	272	14	0.0108	77	5.72	105	290
Embezzlement	0.006	272	14	0.0054	77	5.72	51	140
Stolen property	0.079	420	12	0.1097	138	5.72	1223	3367
Vandalism	0.354	277	8	0.3229	69	5.72	2732	7519
Weapons	0.054	372	16	0.0667	135	4.38	630	1733
Sex offenses	0.025	375	4	0.0307	118	8.18	471	1297
Drug Abuse Violations	0.85	258	8	0.7221	44	4.78	3495	9621
Family/Children	0.035	282	17	0.0328	62	8.18	350	963
Driving Under Influence	0.127	163	6	0.068	21	4.38	229	630
Liquor laws	0.967	163	6	0.518	21	4.38	1735	4777
Drunkenness	0.209	163	6	0.112	21	4.38	375	1031
Disorderly conduct	0.35	207	4	0.2376	26	4.38	775	2134
All other non-traffic	1.412	189	2	0.8735	42	4.38	5050	13900
Curfew & Loitering	0.019	207	4	0.0129	26	4.38	42	115
Subtotal							42730	117615
Total				**5.35**				**74,885**

Notes: Offenses not shown include manslaughter by negligence, prostitution and commercialized vice, gambling, vagrancy and suspicion, for which the arrest rate is 0, and runaways - a status offense which only applies to juveniles. Evaluated at a population of 27221, the mean for treatment agencies. Cost estimates are in 2015 $.

Appendix

A.11.1 Solving the Model

This section calculates the the steady state values of k_t and λ_t, and shows that the system exhibits saddle path stability close to the steady state.

A.11.1.1 Steady State k_t and λ_t

Dynamics in the model can be summarized by the following equations:

$$\dot{k}_t = c_t - \delta k_t = \frac{k_t^\alpha + \lambda_t}{2s_t} - \delta k_t$$

$$\dot{\lambda}_t = (\rho + \delta)\lambda_t - \frac{\alpha c_t}{k_t^{1-\alpha}}$$

At the adult steady state, $\dot{k}_t = 0$

$$c_t = \delta k_t \implies \lambda_t = 2s_t \delta k_t - k_t^\alpha$$

At the adult steady state, $\dot{\lambda}_t = 0$ as well

$$(\rho + \delta)\lambda_t = \frac{\alpha c_t}{k_t^{1-\alpha}}$$

Substituting in $c_t = \delta k_t$

$$(\rho + \delta)\lambda_t = \alpha k_t^\alpha$$

Using $\lambda_t = 2s_t \delta k_t - k_t^\alpha$ and assuming $k_A^{SS} \neq 0$

$$\begin{aligned} (\rho+\delta)(2s_t\delta k_t - k_t^\alpha) &= \alpha k_t^\alpha \\ \implies (\rho+\delta)(2s_t\delta k_t^{1-\alpha} - 1) &= \alpha \\ \implies k_A^{SS} &= [\tfrac{1}{2s_t\delta}\{\tfrac{\alpha}{(\rho+\delta)} + 1\}]^{\frac{1}{1-\alpha}} \end{aligned}$$

The steady state value of criminal capital decreases in criminal sanctions s, depreciation rate δ and the rate at which future utility is discounted δ. However, k_A^{SS} increases with the returns to additional criminal capital, represented by α.

A.11.1.2 Saddle Path Stability

To show that the system of differential equations exhibits saddle path stability, I use a first order Taylor approximation to linearize the system around the steady state values. This system can be written in matrix form:

$$\begin{bmatrix} k_t \\ \lambda_t \end{bmatrix} \approx \begin{bmatrix} \frac{\alpha(\rho+\delta)-(\alpha+\rho+\delta)}{\alpha+\rho+\delta} & \frac{1}{2s_t} \\ (1-2\alpha)(\rho+\delta) + \alpha(1-\alpha) & (\rho+\delta)(1 - \frac{\delta\alpha}{\alpha+\rho+\delta}) \end{bmatrix} \begin{bmatrix} k_t - k^* \\ \lambda_t - \lambda^* \end{bmatrix} = [A] \begin{bmatrix} k_t - k^* \\ \lambda_t - \lambda^* \end{bmatrix}$$

The necessary and sufficient condition for saddle-path stability is that the determinant of A is negative. This condition is met if $0 < \alpha < \frac{1}{2}$ since

$$\frac{\alpha(\rho+\delta)-(\alpha+\rho+\delta)}{\alpha+\rho+\delta} < 0$$

$$\frac{1}{2s_t} > 0$$

$$(1-2\alpha)(\rho+\delta) + \alpha(1-\alpha) > 0$$

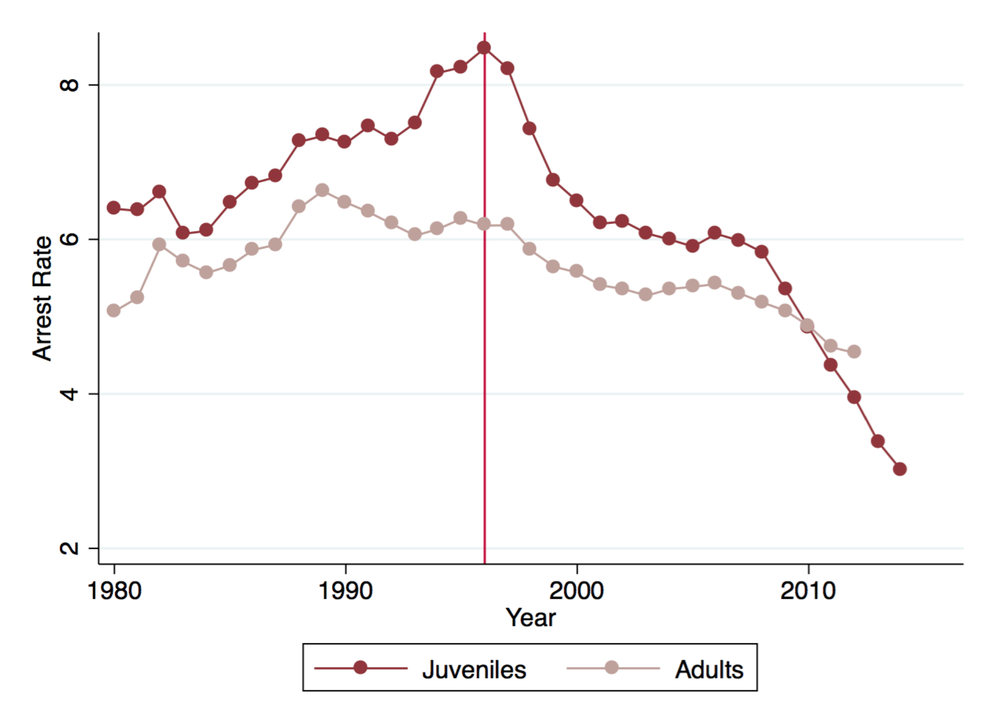
$$(\rho+\delta)(1-\frac{\delta\alpha}{\alpha+\rho+\delta}) > 0$$

However, this is a subset of the parameter values that satisfy the condition $|A| < 0$. Values of (α, ρ, δ) that satisfy $(1-2\alpha)(\rho+\delta) + \alpha(1-\alpha) > 0$ also guarantee saddle path stability.

A.11.1.3 k_{min}

$$\begin{aligned}
\dot{\lambda}_t &= 0 \\
\implies \lambda_t &= [\frac{\alpha}{2S_J} k_t^{2\alpha-1}]/[\rho+\delta-\frac{\alpha k_t^{\alpha-1}}{2S_J}] \\
\implies \lambda_t &= \frac{\alpha k_t^{\alpha}}{2S_J(\rho+\delta)k_t^{1-\alpha}-\alpha} \\
&\to \infty \\
\text{as} \quad & k_t \to \frac{\alpha}{2S_J(\rho+\delta)}^{\frac{1}{1-\alpha}} = k_{min}
\end{aligned}$$

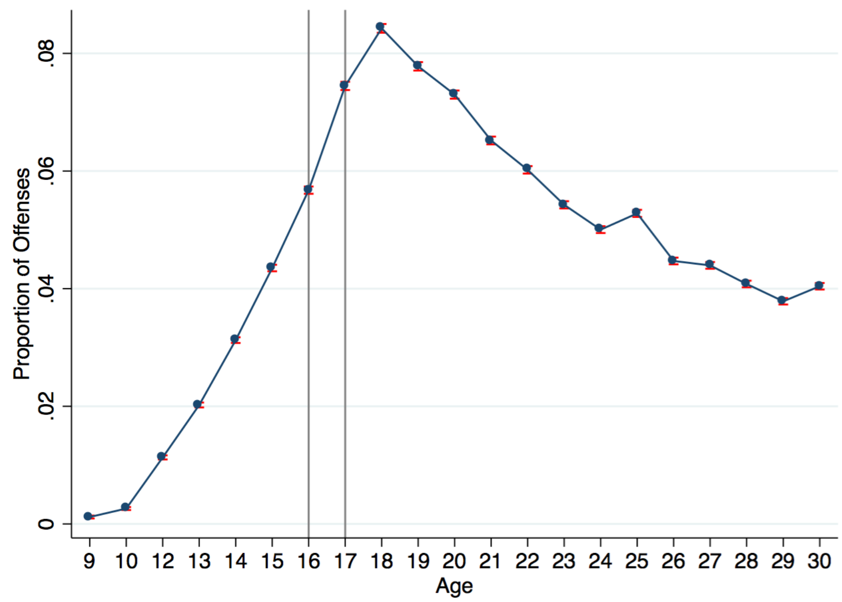
FIGURE A.1: JUVENILE AND ADULT ARREST RATES 1980-2014



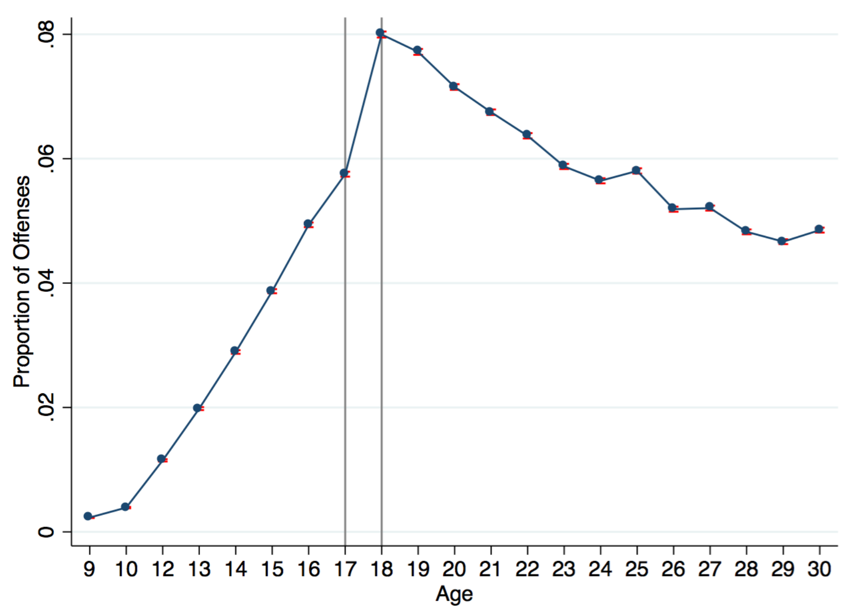
Notes: Based on data released by the Office of Juvenile Justice and Delinquency Prevention.

FIGURE A.2: PROPORTION OF OFFENSES BY AGE 2006-14

(A) AGE OF CRIMINAL RESPONSIBILITY = 17

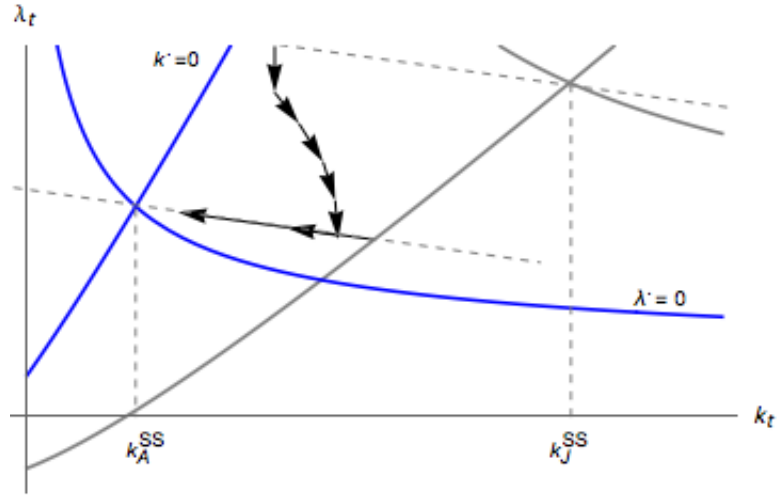


(B) AGE OF CRIMINAL RESPONSIBILITY = 18



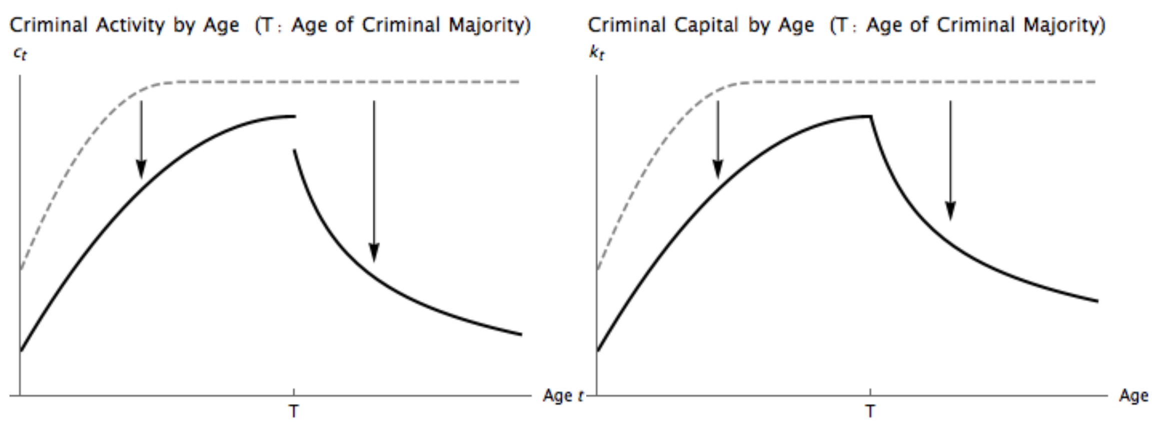
Notes: This graph uses monthly data at the law enforcement agency level from 39 states in the NIBRS data. Confidence intervals are shown in red.

FIGURE A.3: CRIMINAL CAPITAL ACCUMULATION UNDER ANTICIPATED ADULT SANCTIONS



Notes: This figure presents an alternative path for k_t that is consistent with optimizing behavior.

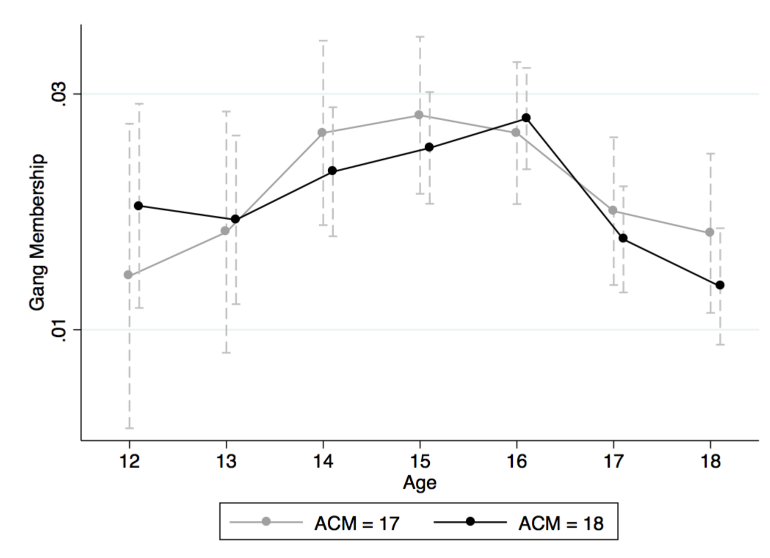
FIGURE A.4: c_t AND k_t UNDER ANTICIPATED ADULT SANCTIONS



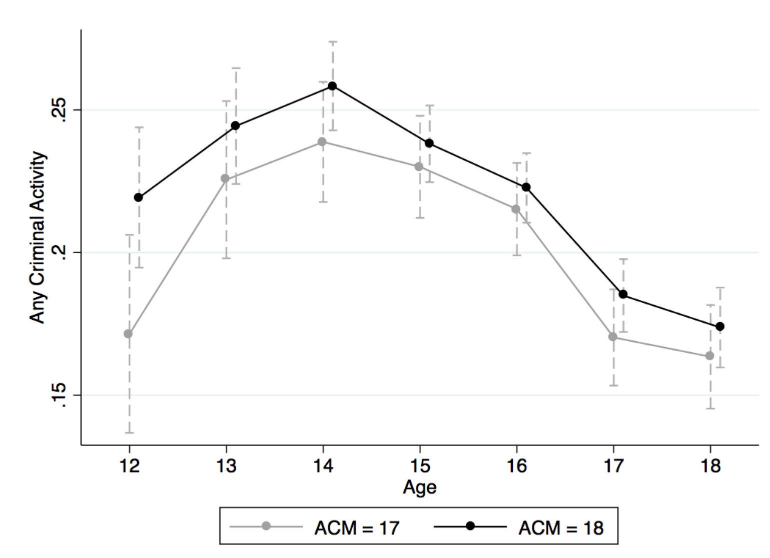
Notes: This figure displays optimal paths for c_t and k_t under the scenario displayed in the above phase diagram. The dashed line marks the optimal paths for c_t and k_t if sanctions stay fixed at S_J.

FIGURE A.5: AGE PROFILES OF GANG MEMBERSHIP AND CRIMINAL INVOLVEMENT

(A) GANG MEMBERSHIP



(B) CRIMINAL INVOLVEMENT



Notes: This graph uses the NLSY97 self-reported data on gang membership and criminal involvement. The coefficients are estimates from a regression of gang membership on age-fixed effects.

Table A.1: States' Age of Criminal Majority Over Time

State	ACM in 2017	Changes
Alabama	18	16 until 1975, 17 until 1976
Connecticut	18	16 until 12/31/2009, 17 until 6/30/2012
Illinois	18	17 for misdemeanours until 12/31/2009 17 for felonies until 12/31/2013
Louisiana	18	17 until 2016
Massachusetts	18	17 until 9/18/2013
Mississippi	18	17 for misdemeanors until 6/30/2011[a] Still 17 for some felonies
New Hampshire	18	18 until 1996, 17 until 6/20/2015
New York	16	Will change to 17 on 10/1/2018 change to 18 on 10/1/2019
Rhode Island	18	18 until 30/6/2007, 17 until 11/7/2007
South Carolina	18	17 until 2016
Wisconsin*	17	18 until 1996
Wyoming	18	19 until 1993
Alaska, Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Florida, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Maryland, Minnesota, Montana, Nebraska, Nevada, New Jersey, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Dakota, Tennessee, Utah, Vermont, Virginia, Washington, West Virginia	18	-
Georgia, Michigan*, Missouri*, Texas*	17	-
North Carolina*	16	-

*Legislation introduced to raise ACM, not succeeded to date: Wisconsin AB387 introduced 9/23/13, failed 4/8/14; Texas: HB 122 introduced 11/14/16, passed House on 4/20/17; North Carolina: HB 725, introduced 4/10/13, passed House on 5/21/14; Missouri: HB 274 introduced 12/19/1; Michigan: HB 4607 introduced 5/11/7.

[a] https://www.ncjrs.gov/pdffiles1/ojjdp/232434.pdf

TABLE A.2: JURISDICTIONS BORDERING TREATMENT AND CONTROL STATES

State	Border Municipalities	Police Agencies
Connecticut	Salisbury, Sharon, Kent, Sherman, New Fairfield, Danbury, Ridgefield, Wilton, New Canaan, Stamford, Greenwich	Connecticut State Police, Danbury, Ridgefield, Wilton, New Canaan, Stamford, Greenwich
Massachusetts	Williamstown, Hancock, Richmond, West Stockbridge, Alford, Edgemont, Mount Washington, Clarksburg, Monroe, Florida, Rowe, Heath, Colrain, Leyden, Bernardston, Northfield	Wiliamstown, Egremont, State Police: Berkshire County, State Police: Franklin County, Bernardston
New York	Petersburg, Berlin, Stephentown, Northeast (Millerton), Amenia, Dover, Pawling, Patterson, Southeast (Brewster), North Salem, Lewisboro, Pound Ridge, North Castle, Harrison, Rye Brook, Port Chester	Millerton, Rensselaer, Brewster, Lewisboro, Pound Ridge, North Castle, Harrison, RyeBrook, Port Chester, Dutchess, Putnam, Westchester Public Safety
Vermont	Canaan, Lemington, Bloomfield, Brunswick, Maidstone, Guildhall, Lunenburo, Concord, Waterford, Barnet, Rye Gate, Newbury, Bradford, Fairlee, Thetford, Norwich, Hartford, Hartland, Windsor, Weathersfield, Springfield, Rockingham, Westminster, Putney, Dummerston, Brattleboro, Vernon, Guilford, Halifax, Whitingham, Readsboro, Stamford, Pownal	Canaan, State Police: St. Johnsbury, Bradford, Thetford, Norwich, Hartford, State Police: Royalton, Windsor, Weathersfield, Springfield, State Police: Bratteboro, Bratteboro, Vernon, State Police: Shaftsbury

Table A.3: Robustness Checks: Impact of an Increase in the Age of Criminal Majority on Juvenile Arrest Rate for Gang Related Offenses

Sample	6 NE States	6 NE States	6 NE States	6 NE States Excl. Boundary Jurisdictions	5 NE States (Excl. CT)	9 NE States	Entire US[a]
DDD Estimate	0.337 (0.109)***	0.453 (0.105)***	0.221 (0.110)**	0.338 (0.114)***	0.490 (0.136)***	0.484 (0.126)***	0.540 (0.152)***
95% C.I. Clustering Level: Age-State	[0.123, 0.551]	[0.242, 0.664]	[0.002, 0.440]	[0.114, 0.562]	[0.222, 0.765]	[0.237, 0.731]	[0.242, 0.838]
95% C.I. Clustering Level: Juvenile-State	[0.097, 0.592]	[0.261, 0.668]	[-0.054, 0.494]	[0.109, 0.573]	[0.243, 0.734]	[0.143, 0.851]	[0.058,1.022]
Mean	3.925	3.925	3.925	3.971	3.927	4.259	5.235
Control Age Groups	[22, 65)	[22, 30)	[30, 65)	[22, 65)	[22, 65)	[22, 65)	[22, 65)
Observations	945,000	504,000	693,000	916,200	817,200	2,453,400	8,314,200

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Juveniles are defined as 13-17-year-olds. Confidence intervals are estimated using the wild bootstrap when the number of clusters is below thirty. *** p<0.01, ** p<0.05, * p<0.1.

[a]Sample consists of 49 states in the UCR

TABLE A.4: ROBUSTNESS CHECKS: IMPACT OF AN INCREASE IN THE AGE OF CRIMINAL MAJORITY ON JUVENILE CRIME INDEX FOR GANG RELATED OFFENSES

Sample	6 NE States	6 NE States	6 NE States	6 NE States Excl. Boundary Jurisdictions	5 NE States (Excl. CT)	9 NE States	Entire US[a]
DDD Estimate	0.019 (0.003)***	0.017 (0.003)***	0.018 (0.003)***	0.018 (0.003)***	0.022 (0.004)***	0.020 (0.003)***	0.009 (0.003)***
95% C.I. Clustering Level: Age-State	[0.013, 0.025]	[0.011, 0.023]	[0.012, 0.024]	[0.012, 0.024]	[0.014, 0.029]	[0.014, 0.026]	[0.003, 0.015]
95% C.I. Clustering Level: Juvenile-State	[0.011, 0.026]	[0.010, 0.025]	[0.010, 0.027]	[0.009, 0.026]	[0.014, 0.031]	[0.012, 0.028]	[-0.0002, 0.019]
Mean	-0.028	-0.028	-0.028	-0.029	-0.027	-0.010	-0.003
Control Age Groups	[22, 65)	[22, 30)	[30, 65)	[22, 65)	[22, 65)	[22, 65)	[22, 65)
Observations	945,000	504,000	693,000	916,200	817,200	2,453,400	8,314,200

Notes: Regressions estimate the impact of an increase in the Age of Criminal Majority from 17 to 18. Juveniles are defined as 13-17-year-olds. Confidence intervals are estimated using the wild bootstrap when the number of clusters is below thirty. *** p<0.01, ** p<0.05, * p<0.1.

[a]Sample consists of 49 states in the UCR

Chapter 2

The Career Impact of First Jobs: Evidence and Labor Market Design Lessons from Randomized Choice Sets*

Ashna Arora† Jonas Hjort‡

Abstract

We study the impact of a graduate's first job on her career trajectory; job-seeking graduates' response; and how policy rules' impact on worker-job matches ultimately helps shape the distribution of realized "first job effects" (FJEs). In Norway, doctors' first job—their residency—is allocated through a random serial dictatorship (RSD) mechanism. We first exploit the resulting random variation in individual doctors' choice sets to estimate each type of job characteristic's impact on long-run earnings, place of residence, and specialization for each type of individual. We then account for these FJEs when estimating each worker type's preferences over job characteristics, enabling us to decompose the long-term consequences of a particular choice set into a component that is due to chance and a component that is due to workers' preference-driven response. Finally, we exploit the replacement of the RSD mechanism with decentralized job-finding in 2013 to show how total worker welfare and the distribution of *realized* FJEs across worker types—due to changes in worker-job matches—differ in a market system relative to RSD. To our knowledge, these findings represent the first causal evidence on the career impact of individual level variation in first jobs and the challenges and opportunities they present for graduates and policymakers.

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1 Introduction

An individual's first job may have important consequences for her career trajectory. This view—common among researchers—appears widely held also among those entering the labor market.[1] New job-seekers may therefore put weight on the expected impact of different jobs on their trajectories when choosing a first job to pursue. Policy, on the other hand—whether centralized mechanisms allocating doctors, teachers, and other groups of workers serving the public to first jobs[2], or the rules and regulations that influence initial worker-job matches in the decentralized labor market—is typically designed without accounting for expected "first job effects" (FJEs). If FJEs are non-negligible in magnitude and heterogeneous across types of workers, then FJE-responsive policy design *may* increase welfare. However, even in such a scenario, whether alternative policies *actually* affect initial worker-job matches—and hence realized FJEs—differentially is an empirical question. Unusual types of data and variation are necessary for researchers to be able to identify FJEs and graduates' and policymakers' actual and ideal response. To estimate the causal, long-term effect of an individual's first job, random variation in her match, holding all else constant, is needed. To go beyond such a LATE and estimate how individuals' distribution of FJEs across jobs influence their job search choices, causal estimates of the long-term effect of each type of job for each type of individual—and knowledge of individuals' choice set when entering the labor market—are needed.

In this paper we take advantage of Norway's 1997-2013 allocation of doctors' first job—their residency—through a Random Serial Dictatorship (RSD) mechanism[3], and the replacement of the RSD with decentralized job-finding in 2013, to overcome these challenges. We first estimate the consequences for earnings, place of residence, and specialization in the long-term of each type of job characteristic for each type of individual. We do so by exploiting RSD-generated random, individual level variation in new doctors' choice sets over jobs. We then account for these FJEs when

[1] The influential studies by Genda *et al.* (2010); Heisz *et al.* (2012); Kahn (2010); Oyer (2006, 2008) all document persistent career effects for cohorts of new workers that graduate in a weak labor market.

[2] The following is an incomplete list of countries that use centralized mechanisms to assign workers in some (in some of the countries, almost all) public service occupations to first jobs: Australia, Bangladesh, Bhutan, Botswana, Canada, Denmark, France, Ghana, India, Iran, Ireland, Israel, Italy, Japan, Malaysia, Malta, Nepal, Norway, Pakistan, Philipines, Saudi Arabia, Senegal, Singapore, South Africa, South Korea, Taiwan, Tanzania, Uganda, U.K., USA.

[3] A Random Serial Dictatorship mechanism starts with a lottery. The person who draws number 1 then chooses her preferred object freely among all available options. After that, the person who draws number 2 chooses among the remaining objects, and so on.

estimating each type of individual's preferences over (short- and long-term) job characteristics.

The unique suitability of doctors' residencies in Norway for studying FJEs is due to the combination of choice sets over jobs being assigned randomly, and the unusually high quality of the registry data available on the universe of Norwegian workers. While our quantitative results may not generalize to other occupations, it is worth noting that (i) the differences between the possible pathways a doctor's career can take share many features with those in other occupations[4], and (ii) the literature generally finds that highly skilled workers are least affected by temporary career shocks (Oreopoulos *et al.* , 2012; von Wachter & Bender, 2006). Most likely our results thus represent a lower bound on FJEs and the associated responses in other occupations.

This paper contributes to the literature on how temporary shocks to a worker's employment status affects her career trajectory. Existing studies have convincingly and carefully documented the consequences of job displacement (see, among many others, Bender *et al.* , 2009; Sullivan & von Wachter, 2009; von Wachter & Bender, 2006); exposure to high unemployment rates later in life (Coile *et al.* , 2012); and, most closely related to this paper, graduating in a weak labor market (Genda *et al.* , 2010; Heisz *et al.* , 2012; Kahn, 2010; Oyer, 2006, 2008). These influential studies have shown how cohort and group-level labor market shocks affect individual workers' trajectories.[5] In addition to taking advantage of an explicit randomization for identification, this paper to our knowledge provides the first causal evidence on the career consequences of *individual* level shocks to a graduate's first job. The distinction is essential because cohort level studies may not be informative about the career consequences of individual level labor market shocks, which are ubiquitous. When the cohort or group an individual belongs to is hit, for example, by a recession or a mass layoff, then the individual's peers are also affected. Peers' exposure to the shock could adversely affect the trajectory of the individual in question (if for example she now faces more competition for current jobs) or benefit her (if for example she's now competing against less employable other workers for future jobs).[6]

[4]For example, doctors' jobs are located in many different parts of a country; there is considerable dispersion in employer size and "quality" (which is highly correlated with doctors' earned income); and there are ample opportunities for doctors to undertake horizontal specialization (choice of medical field) as well as vertical specialization (e.g. becoming a specialist as opposed to a General Practitioner).

[5]To our knowledge, the only existing causal evidence on the long-term effects of individual level shocks to first jobs comes from Angrist (1990)'s seminal study of the Vietnam draft. He shows that being drafted lowered earnings by 15 percent long after the veterans' service ended (see also Angrist, 1995; Angrist & Chen, 2011).

[6]Ruhm (2000) shows that mortality tends to improve during recessions, while Sullivan & von Wachter (2009) show that *own* job displacements increase mortality for U.S. workers.

Our second contribution is to identify how graduates respond to variation in the labor market they face. To do so we estimate graduates' preferences over job characteristics. What allows us to do so is the fact that we observe each graduate's complete choice set over jobs, and that the choice set is de facto randomly determined. With estimates of graduates' preferences in hand we can decompose the long-term consequences of a particular choice set into a component that is due to *chance* and a component that is due to workers' *response*. This allows us to estimate how graduates' welfare is affected by labor market shocks. The existing literature has studied the impact of recessions, displacement, and other temporary shocks on particular components of workers' future welfare, such as income. These impacts are important, but do not tell the complete story since workers value other job characteristics as well.[7]

The paper is organized as follows. In Section 2 we discuss background on the setting and institutional setup. In Section 3 we present the datasets used in our empirical analysis. In Section 4 we lay out our empirical strategy. Section 5 estimates doctor preferences for residency characteristics, shows how residency characteristics influence doctors' careers in the long term, and how doctors account for these long-term effects when choosing a residency hospital. Section 6 concludes.

2 Setting

2.1 The Random Serial Dictatorship mechanism for Norwegian doctors

The "turnus" (roster) system that was used to match medical graduates with residency positions in Norway from 1954 to 2013 was a Random Serial Dictatorship (RSD) mechanism. Theorists have shown that, among other important properties, the RSD is incentive-compatible, inducing participants to reveal their true preferences (Abdulkadiroglu & Sönmez, 1998).

Equitable access to primary healthcare across regions was the main motivation behind the use of a lottery system in Norway. Like other countries, Norway had had trouble filling doctor vacancies in rural areas, and the RSD mechanism was expected to distribute the best doctors more equally across space. In addition, the mechanism appealed to policymakers because it was per-

[7]This paper is of course not the first to recognize that workers care about non-income job characteristics and may choose occupations and employers partly based on those preferences. Recently, for example, Sorkin (2016) showed evidence of compensating differentials revealed in workers' job-to-job transitions in the U.S.

ceived to be fair to the participating medical graduates.[8]

First, graduating students would enter a lottery, either in February or in August, and be assigned a random draw number. Next, the student with the lowest draw number would choose freely between all available positions. Next, the student with the second lowest draw number would choose from the remaining residency positions. This would continue until the student with the highest draw number remained, who would take whichever spot was available.[9]

Three categories of new doctors received special treatment: couples, who were allowed to draw a shared lottery draw number and to choose residencies simultaneously; doctors with children; and doctors with maternity or health issues. The latter two categories were allowed to choose between positions deemed especially suitable for them before the lottery took place. Since these three types of doctors were not subject to randomization via the lottery, we exclude them from our analysis.

In the late 2000s, the system began to concern the government, because of the growth of the number of applicants and the rise in proportion of students from foreign universities.[10] The number of medical graduates participating in the lottery would routinely exceed the number of training positions available. As a result, it became increasingly difficult for the government to guarantee a six-month maximum waiting time to obtain a residency. In 2013, the Norwegian Health Minister replaced the lottery system with direct qualification after six years of medical school. Medical graduates now apply to residencies directly, as in a regular labor market, and hospital trusts are responsible for selection and recruitment.

2.2 Doctors in Norway 1995-2014

This section profiles doctors that worked in Norway during the study period; we go through the data we use in detail in Section 3. Medical students in Norway begin their studies in the Fall or Spring semester, and usually take ten semesters to graduate. Starting in the 1950s, the Norwegian government mandated an eighteen month residency period, after which medical school graduates

[8] The government also wished to incentivize doctors to work in rural locations in other ways. For instance, doctors who agreed to intern at hospitals in the largely rural counties of Sogn og Fjordande and Finnmark could skip the lottery entirely.

[9] If the number of students exceeded the number of residency positions, the unassigned students would get priority in the next lottery.

[10] Norway was compelled by its participation in the EU common labor market system to accept any European medical graduate who could pass a Norwegian language test into the system.

could become fully licensed physicians and practice independently. The first twelve months were to be spent at a hospital, while the remaining six months were to be spent as a General Practitioner (i.e. one who works in Primary Care) within the same county.[11]

Table 2 summarizes a range of socioeconomic information on doctors including age, proportion born abroad, proportion that studied abroad, family size, field of specialization and income and assets. The last two columns summarize this information separately for men and women. Women comprise over 40 percent of doctors, and tend to be over-represented in fields like gynecology and psychiatry. Male doctors are older and tend to be over-represented in fields like surgery and internal medicine. A fifth of all doctors were born abroad, of which an overwhelming proportion are citizens of Denmark and Sweden. Finally, recorded income, asset and debt holdings are higher on average for male doctors.

There are 30 basic medical specialties, and specialization is usually in the form of training on the job.[12] The average length of time required to complete a specialty is five years, but it can take longer with large variations between the specialties. Figure A.4 indicates that there appear to be substantial returns to specializing—job retention rates are higher for specialists, and the salary bump from specialization increases with age. In Section 5 we show that residency characteristics significantly impact when and whether doctors become specialists.

2.3 Hospitals in Norway 1995-2014

The employer-employee database contains information on all registered employers that employ doctors in Norway.[13] Figure A.3 depicts the steady growth in both the number of hospitals and average hospital size (number of doctors employed) since 1995.

Hospitals vary across multiple dimensions. Table 1 summarizes information on salaries, geographical remoteness, number of doctors and other medical staff, proportion of specialists, as well as the presence of fifteen distinct specialist fields. Most hospitals in Norway are located in urban municipalities; on average, municipalities with hospitals have only 10 per cent of their population living in rural areas. This is noteworthy because the average municipality in Norway had 49

[11] The last six months could be spent at an institution that was disjoint from the hospital of the first twelve months.

[12] The Norwegian Medical Association evaluates whether the candidate has met the requirements to become a specialist. Specialist titles are formally awarded by the Health Directorate.

[13] We define hospitals as employers that hire at least 10 doctors. These account for around 80 per cent of doctor employment.

percent of its population living in rural areas.

Table A.1 shows how seven major hospital characteristics vary with one another. Salaries are typically higher at hospitals that are more specialized. Average salary also increases with remoteness of location.

Post-residency salaries may be used both to compensate doctors to work in less desirable hospitals, and also to attract the best doctors. We now shed some light on how salaries vary with individual and hospital characteristics by estimating the following equation:

$$S_{iht} = \beta_0 + \boldsymbol{\beta X_{ht}} + \boldsymbol{\omega Z_{it}} + \gamma_t + \varepsilon_{iht}$$

S_{iht} is the salary paid to doctor i by hospital h in year t, and X_{ht} is a vector of time-varying hospital characteristics. $\boldsymbol{Z_{ht}}$ is a vector of time varying individual-level characteristics like age, sex, citizenship and choice of specialization (if any). γ_t are year fixed effects. Table A.2 displays the estimates for $\boldsymbol{\beta}$ and $\boldsymbol{\omega}$. Hospitals that are more specialized and located in rural areas pay higher salaries. We include dummies for four categories of hospital size and find that salaries increase with hospital size even within hospitals that employ less than 10 doctors. Turning to individual characteristics, we find that male doctors and those born in Norway earn higher salaries on average. The highest paying specialties include anesthiology, gynaecology, surgery and diagnostic medicine.

3 Data

We combine information on lottery outcomes with Norwegian administrative data from 1993 to 2013. We obtained information on lottery draw numbers for all lottery participants who were assigned a residency position during 1993-2013 from the Norwegian Registration Authority for Health Personnel (SAFH). This information was linked with the employer-employee registry to match medical graduates to their residency hospitals, as well as employer information in the years following the residency. This data was then linked to administrative registers provided by Statistics Norway, a rich longitudinal database that includes information on medical graduates' socioeconomic information (sex, age, marital status, educational attainment, specialization, income, and gross wealth), geographical identifiers and year-end asset holdings and liabilities (such as real estate, stock holdings, etc) for each year. These data have several valuable attributes. There is no attrition from the sample, and most components of income and wealth are third-party reported without any top or bottom coding.

The final dataset tracks the career path of each graduate, starting with her lottery number and choice of residency hospital. After excluding people belonging to special lottery categories and hospitals with missing information, we end up with a sample of about 8000 individuals and 60 hospitals, which participate in 34 lotteries from 1996 to 2013.[14] Figure 1 displays the number of individuals and hospitals that participated in each lottery. Figure A.1 splits participants by gender and by birth location. It is evident that there is an increase in the proportion of women and foreign students over time. Most foreign doctors are citizens of the European Economic Area (EEA). Most medical graduates have a sizable number of residency options to choose from, as displayed in Figure A.2.

We observe employment outcomes for all doctors up until the year 2014. This allows us to track doctors who graduated in the earliest lotteries (during the 1990s) for over fifteen years, while participants in the last few lotteries (in the 2010s) can only be tracked for a few years. Figure 2 displays the distribution of the number of times doctors are observed in the years following their residencies. Our sample consists of roughly 4500 individuals five years after their residency, but less than a quarter of these are observed 10 years down the line.

We construct the choice set of hospitals faced by each lottery participant using her lottery number and the residency hospitals chosen in that lottery. We know that if a hospital h was chosen by someone with a higher (worse) lottery number than individual i, i must have been given the option of choosing h as well (since hospitals cannot reject applicants). Assuming that no residency spots were left unfilled,[15] we can accurately predict the choice sets that were offered to each lottery participant. Formally, suppose we index individuals by increasing order of their lottery number i, and label the hospital chosen by i as h_i, and the choice set of i as C_i. Then, if $i < i'$ it must be true that $h_{i'} \in C_i$. That is, if someone chose a hospital after individual i, it must be the case that their hospital of choice was available to i (since hospitals cannot reject applicants). Therefore, we define choice sets using the fact that $h_{i'} \in C_i \, \forall \, i' > i$.

[14] Data is missing for the lottery in January 1998.

[15] This assumption is reasonable, in part because excess demand was one of the reasons for replacing the turnus system after 2013.

4 Empirical Strategy

This section describes how we use the lottery system to generate viable instruments for doctors' first job. Random lottery draws ensure that the choice set of any given medical graduate is independent of her observable and unobservable characteristics. However, the choice of which hospital to intern at, from within the randomly assigned choice set, is likely to be affected by and correlated with the individual's attributes.

We leverage the fact that any given medical graduate is constrained to choose a residency hospital from within her randomly assigned choice set. That is, instruments based on the choice set are likely to be *relevant*. It is also reasonable to assume that the *exogeneity* condition is satisfied, since the lottery assigns choice sets independently of individual characteristics.

Since we observe a wealth of information about hospitals, we instrument directly for characteristics of residency hospitals. That is, we examine whether features like the degree of specialization of the residency hospital can influence doctors' decisions later in their careers, such as when to specialize themselves. While one could instrument for the residency hospital as a whole,[16] characteristics of hospitals in Norway changed considerably over the 20-year study period. This makes it difficult to interpret and/or decompose estimates of the long-term effects of hospital indicators.

4.1 Instrumented job characteristics

This section describes the construction of instruments for a given residency hospital characteristic. Let x_h^i denote a characteristic of the hospital h that doctor i interned at, and let Y_{it} denote an outcome of of interest for i in year t. We are interested in examining the impact of x_h^i on Y_{it} for various values of t:

$$Y_{it} = \beta_0 + \beta_1^t x_h^i + \varepsilon_{it}$$

The empirical challenge is to control for the fact that interns who choose high values of x_h may be different from those who choose low x_h, i.e. $x_h^i \not\perp \varepsilon_{it}$. We can instrument for x_h^i using summary statistic information on x_h within the randomly assigned choice set C_i. For example, let x_h be the

[16] In this case, instruments would—ignoring weak instruments issues that arise when we instrument for hospitals themselves—satisfy the *exclusion* condition as well, i.e. that long-term outcomes are influenced by the choice set *only* through its effect on initial placement. The fact that our instruments for hospital characteristics are unlikely to satisfy the exclusion in a strict sense—because hospitals are bundles of correlated characteristics—is of less relevance for the exercise in this paper than it would be if one was e.g. considering providing hospitals with new amenities.

proportion of specialists at hospital h. Then summary statistics like the mean, median and modal proportion of specialists at hospitals within C_i (denoted by $S[x_h|h \in C_i]$) satisfy the exogeneity condition:

$$C_i \perp \varepsilon_{it} \implies S[x_h|h \in C_i] \perp \varepsilon_{it}$$

We also need to ensure that our instruments are relevant predictors for the endogenous regressor x_h^i:

$$E[S[x_h|h \in C_i]' x_h^i] \text{ has full rank}$$

4.2 Instrument refinements

The instrument described above makes use of information about *all* hospitals in intern i's choice set C_i. However, by exploiting the fact that we observe choices made by interns other than i, it is possible to develop instruments that are less coarse. The intuition behind refining the instrument is that interns are likely to have a preference ordering over all hospitals in C_i, which if directly observed could be used to generate stronger instruments. For instance, summary statistics of an intern's most preferred hospitals within her choice set are likely to be better predictors of x_h^i than summary statistics of all hospitals in her choice set C_i. Since we do not directly observe preference orderings, we attempt to predict them based on choices made by interns other than i.[17] Armed with preference estimates, we can develop instruments that are more finely tailored to each individual's observable characteristics, which is likely to give us a more powerful first stage. We now describe the model of latent indirect utility that we use to generate preference estimates, and then use them as a basis to design two instrument refinements.

4.3 Intern preferences

Following the discrete choice literature (Berry & Pakes, 2007), we model the latent indirect utility that doctors derive from their first job as a function of hospital observables z_{ht} and individual observables x_{it}. Taste parameters β_{it} reflect heterogeneity in preferences due to observable individual characteristics and are estimated separately for each lottery t to allow for temporal variation in preferences.

[17] We constrain this exercise to individuals participating in the same lottery since intern preferences may evolve substantially over time.

$$u_{iht} = z_{ht}\beta_{it} + \xi_{iht} \qquad (4.2)$$

$$\text{where} \qquad \beta_{it} = \beta_t + \eta_i + x_{it}\Pi_t$$

$$F(\xi_{iht}) = \frac{\xi_{iht}}{1+(\xi_{iht})} \qquad \xi_{iht} \perp z_{ht}$$

The model is estimated using McFadden (1974)'s conditional logit model, which allows C_i to vary across individuals. We estimate β_{it} separately for each individual i using information on all participants other than i within the same lottery t. Excluding i from the estimation of β_{it} is an intuitive step that preserves the exogeneity of our instruments.[18]

4.3.1 Refinement 1: Top choices

This section uses information on predicted ranks of hospitals within each individual's choice set C_i to refine instruments for the chosen hospital characteristic x_h^i. Let $C_i(r)$ denote the set of r highest ranked hospitals in C_i. We can refine the instrument $S[x_h|h \in C_i]$ described above by replacing it with $S[x_h|h \in C_i(r)]$ for some small value of r, i.e. instead of using the summary statistics of all hospitals in C_i, we only use the summary statistics of the r highest ranked hospitals in C_i. The intuition behind this refinement is that x_h^i is more likely to be influenced by i's best choices within C_i, as opposed to being equally influenced by every element in C_i. It is reasonable to assume that the instrument satisfies the exogeneity condition, since $S[x_h|h \in C_i]$ only depends on i's randomly assigned choice set and preferences of other lottery participants.

We test whether this exercise increases the relevance of our instruments by plotting the first stage F-statistic against differing values of r. In Section 5.3.1 we show that using summary statistics of the best ranked hospitals strengthens the instruments.

[18] Suppose i overvalues rural hospitals relative to her peers. Including i in the estimation procedure will lead to a high value of β_{it} and a high predicted probability of i choosing a rural residency hospital. However, i's underlying preference would translate into a long-run outcome of working in rural locations. Our instrument would then incorrectly attribute this to the fact that i interned at a rural hospital. To circumvent this source of endogeneity, we use leave-one-out regressions to to estimate β_{it} separately for each lottery participant i.

4.3.2 Refinement 2: Predicted probabilities

An alternative refinement is to use the predicted values of x_h^i based on our estimates of medical graduates' preferences. Our preference estimates can be used to predict the probability that any given hospital is chosen by i, denoted by $p_i(h)$. Using these probabilities, we can calculate the predicted value of x_h^i as follows:

$$z_h^i = E[x_h^i] = \sum_{h \in C_i} p_i(h).x_h$$

In line with the previous subsection, we can refine this instrument even further by focusing on $C_i(r)$ instead of C_i for small values of r. Define $z_h^i(r)$ as the expected value of x_h^i, conditional on i choosing a hospital within her top r choices.

$$z_h^i(r) = \frac{1}{\sum_{h \in C_i(r)} p_i(h)} \sum_{h \in C_i(r)} p_i(h).x_h$$

$z_h^i(r)$ is likely to satisfy the exogeneity condition because it only depends on i's randomly assigned choice set and preferences of other lottery participants. Section 5.3.1 empirically verifies that $z_h^i(r)$ is a relevant instrument, and shows how first stage relevance varies with r.

4.4 Endogenous variables: multiple hospital characteristics

The instruments described above provide causal estimates of the impact of a single first job characteristic on long-run outcomes for medical residents, by attenuating the bias due to omitted individual characteristics. However, residency hospitals are bundles of non-separable characteristics that vary simultaneously as shown in Table A.1. For instance, high salaries and rurality of location are positively correlated, and it is possible that we are picking up the effects of high salaries when we instrument for rural location.

Before we proceed with our results, we describe briefly the interpretation of the $\boldsymbol{\beta}$ coefficients on the instrumented variables $\boldsymbol{x_h}$. Hospitals are bundles of characteristics, of which only a subset are observable. β_x (the coefficient on characteristic x) may be picking up the effects of unobservable hospital characteristics. For instance, assume larger hospitals tend to attract high quality doctors (which we do not observe), and that doctors value co-worker quality. We may observe higher retention rates for those working at larger hospitals, not because of the large size per se, but because of the associated increase in co-worker quality. Therefore, our estimates tell us whether we should expect residency hospital characteristic x_h to influence long-term outcomes of interns, but not whether it is x_h itself or something unobservable that is correlated with x_h that is driving the persistence results. What our instrument does do is to eliminate bias in the persistence estimates due to individual unobservables.

We can instrument for multiple hospital characteristics using a straightforward extension of Section 4.1. Let $\boldsymbol{x_h^i}$ denote a vector of characteristics of the hospital h that doctor i interned at, and let Y_{it} denote an outcome of of interest for i in year t. We are interested in examining the impact of $\boldsymbol{x_h^i}$ on Y_{it} for various values of t.

$$Y_{it} = \beta_0 + \boldsymbol{\beta_1 x_h^i} + \varepsilon_{it}$$

The empirical challenge is to account for the fact $\boldsymbol{x_h^i}$ may be endogenous:

$$\boldsymbol{x_h^i} \not\perp \varepsilon_{it}$$

We can instrument for $\boldsymbol{x_h^i}$ using summary statistic information on $\boldsymbol{x_h}$ within the randomly assigned choice set C_i, denoted by $S[\boldsymbol{x_h}|h \in C_i]$. We can strengthen these instruments by using the refinements described in Section 4.1. To test for under-identification and weak instruments, we use the heteroskedasticity-robust Kleibergen & Paap (2006) Wald rk F-statistic[19] Although the appropriate critical values in the heteroskedastic case have not been tabulated in the literature (Mikusheva, 2013), we follow the common practice of comparing this statistic to the Stock & Yogo (2005) critical values.

5 Results

This section first verifies that lottery numbers were in fact randomized across lottery participants under the turnus system. We then estimate preferences for hospital characteristics and use them to create viable instruments for residencies chosen by medical graduates. These instruments are used to show that residency characteristics have persistent effects on doctors' careers, captured by earnings, place of residence, and specialization. Next, we show that medical graduates value these long-term outcomes when deciding between residencies, and that at least some of the persistence documented above is "chosen" by graduates. Finally, we show that the replacement of the RSD

[19] Instead of the Cragg & Donald (1993) F-statistic which assumes homoskedasticity.

mechanism with decentralized job-finding in 2013 altered the distribution of worker welfare across worker types, and total worker welfare.

5.1 Randomization

First, we test that the lottery was able to successfully randomize draw numbers. A formal test is that individual characteristics ζ_i should not affect her lottery draw number D_{iL}.

$$D_{iL} = \beta\zeta_i + \gamma_L + \varepsilon_{iL}$$

We normalize lottery draws to lie on the interval $[0, 1]$ by dividing by the largest draw number. We also include lottery fixed effects to account for lottery specific shocks such as larger participation overall, or larger participation by particular demographics in a given lottery. Table 3 displays the results of regressions for five individual characteristics: rurality of hometown[20], gender, age, whether the participant was born abroad, and whether he studied abroad. None of these characteristics are significant predictors of an individual's lottery draw number.[21]

5.2 Doctor preferences

This section describes our estimates of lottery participants' preferences for residency characteristics. We divide interns on the basis of gender, birth country, and rurality of hometown (if born in Norway), and estimate preferences separately for each group.[22]

Table A.3 displays preference estimates based on section 4.3 for the following hospital characteristics: log salary,[23] rural location, proportion of specialists, number of specialists in internal medicine, surgery, diagnostic medicine and psychiatry,[24] as well as hospital size (log number of doctors). Interns across demographic groups value larger hospitals, reflected by the significant and positive coefficient on hospital size. To enable comparisons across demographic groups, we restate

[20] Hometown is defined as the municipality at age 15, and rurality measures the proportion of rural residents in the municipality.

[21] Results do not change meaningfully if we include all five characteristics in a single regression.

[22] An intern's hometown is classified as rural if, at age fifteen, he lived in a municipality in which the proportion of people living in rural areas was above the median (which is about 10 per cent). If this information is not available, it is replaced by rurality of the municipality at age 5 or municipality at birth. Data on municipality rurality is only available from 1990 onwards, so if the year at age 0/5/15 is prior to 1990, we use the rurality of the municipality in 1990 instead.

[23] An estimate based on the average salary of the previous year's interns at the same hospital.

[24] We have included the number of specialists rather than the existence of the most popular specializations (internal, surgery, diagnostic) since there usually exists at least one specialist in each category at any given hospital, leading to multicollinearity problems in the regression. See Table 1.

intern preferences in marginal willingness to pay terms, i.e. we focus on the ratio at which interns are willing to substitute hospital characteristics for an additional percent increase in hospital size.

These results are displayed in Table 4. As noted above, each entry displays the marginal willingness to pay for a percentage point increase in hospital size. Interestingly, coefficients on residency salary are either negative (most Norwegians) or not significantly different from zero (foreigners and urban Norwegian men).[25] As expected, Norwegians from rural municipalities do not place a negative weight on rural locations, while the reverse is true for foreigners and urban Norwegian women. Male doctors value specialized hospitals (relative to hospital size) more than female doctors. Foreigners and urban Norwegians value hospitals with more surgeons and less internal medicine specialists; rural Norwegians do not display strong preferences towards specific specializations.

5.3 Persistent effects of residency characteristics

This section examines the long-term impact (FJEs) of eight residency characteristics: salary, rurality of location, proportion of specialists, number of specialists in internal medicine, surgery, diagnostic medicine and psychiatry, and hospital size (number of doctors). We instrument for residency characteristics using the strategies detailed in Section 4.2 and show that FJEs on income, specialization and location decisions of doctors can persist for up to ten years.

5.3.1 First stage

Summary statistics (mean, median, variance) of hospital characteristics in C_i sets qualify as instrumental variables for residency characteristics actually chosen by i. We use the refinements described in Section 4.2 to increase their first stage explanatory power. Motivated by the preference patterns documented above, we sort individuals into four demographic groups: foreigners, rural-born Norwegians, urban female Norwegians and urban male Norwegians. Each resident i's choices are assigned probability weights based on the choices of other residents belonging to the same demographic group in the same lottery. We then use these probability weights in two ways. One, we restrict attention to the top seven choices when calculating summary statistics.[26] Second,

[25]The negative coefficients on hospital salary are not economically intuitive, but may be driven by the fact that we do not observe hours worked. Also see Agarwal (2015) for another instance in the literature.

[26]This number is based on the restriction that gives us the most powerful instrument, as displayed in Figure A.5.

we create weighted summary statistics, which prioritize hospitals that are most likely to be chosen by i.

Our first set of instruments consists of the expected values of residency characteristics, as well as the standard deviation of average hospital salary and number of doctors.[27] Figure 3 illustrates how relevance increases when we constrain attention to higher ranks to construct instruments: for each refinement, we plot first stage Kleibergen-Paap Wald rk statistics for the highest rank summary statistics are constrained to.[28] Instrument relevance is highest for weighted summary statistics based on the top seven hospitals of each resident's choice set. Appendix Figure A.5 displays weak instrument tests for alternative summary statistics like quartiles as instruments, which deliver weaker first stages than the one outlined above. Therefore, we use expected values and standard deviations as instruments in the analysis that follows.

5.3.2 Second stage: effects on long-run outcomes

In this section, we instrument for residency characteristics and show that they continue to influence doctors' careers for over ten years after the completion of their residencies.

We focus on three aspects of doctors' careers: earnings, the decision to specialize and location. χ^2 tests are used to test whether FJEs of eight residency characteristics are collectively different from zero. Figure 4 displays these tests for each year following the lottery. We find that FJEs on each of these outcomes continue to be significantly different from zero for over ten years after the residency period.[29] The pattern documented in the middle panel is especially interesting. Most doctors become specialists 8-12 years after their residencies, and the peaked pattern of χ^2 tests suggests that FJEs significantly affect the decision to specialize in fields that take less time, or that FJEs are more significant for the decision to specialize sooner (than average) in one's career. Figure A.6 repeats this exercise separately for the four demographic groups described above: FJEs continue to impact earnings and the decision to specialize well beyond ten years after residency

[27] This enables us to run weak instrument tests, which require the number of instruments to exceed the number of endogenous variables (henceforth, EV) by at least two. The critical value for maximal 10 per cent bias with 1 EV and 3 instruments is 9.08, 2 EV and 4 instruments is 7.56, 3 EV and 5 instruments is 6.61. Stock & Yogo (2005) do not report critical values for more than 3 EV - we assume that the declining trend from the above 3 numbers means that 6.61 is an upper bound for the critical value in the 4 EV 6 instrument case.

[28] Actual first stage statistics will vary, because both stages are jointly estimated in a 2SLS regression.

[29] Tables A.4 - A.6 display the year-by-year effects of each hospital characteristic on post-residency hospital salary, specialization and location.

completion across demographic categories.

To shed light on how residencies affect long-term earnings, we separately examine the FJEs of each residency characteristic. Table A.4 displays these characteristic-specific FJEs on hospital salary for up to fifteen years after the completion of doctors' residencies. Residency salary lowers long-term earnings, which is consistent with our estimates of negative doctor preferences for earnings during the residency period. On the other hand, residencies that are located in rural areas and those in which doctors are exposed to a high proportion of specialists appear to consistently increase long-term earnings, which is most consistent with our preference estimates for rural Norwegians. Last, the number of doctors a resident is exposed to exerts a negative influence on earnings for the first few years following the residency, and then begins to exert a positive influence.

Table A.5 displays these characteristic-specific FJEs on the decision to specialize. Residency salary and rurality lower the probability that a doctor will specialize in most years following her residency. This is consistent with most medical graduates valuing residency earnings negatively. The proportion of specialists that a doctor is exposed to during her residency, however, exerts a uniformly positive influence on the decision to specialize in the long term. Consistent with the pattern of χ^2 tests above, the coefficients are largest in magnitude 8-12 years after doctors enter the labor market.

Finally, we examine the pattern of characteristic-specific FJEs on doctor location in the long term. Table A.6 shows that residencies with high salary and more specialists reduce the expected rurality of location in the long term. On the other hand, residencies in rural areas exert a positive influence on how rural doctors' locations are throughout their careers.

5.4 Doctor preferences for long-term outcomes

In this section, we test whether medical graduates account for the fact that each potential first job is associated with its own set of long-term outcomes, and value these post-residency outcomes when deciding between residencies. We do this by including three "long-term" residency characteristics in our estimation of preferences - post-residency salary, specialization, and rurality of location of doctors that completed the same residency program in previous years. The implicit assumption is that medical graduates are knowledgeable about the career paths of doctors that completed their residencies at each hospital.[30]

Table 5 displays the revised preference estimates, which exhibit interesting patterns for most demographic groups. For instance Urban Norwegian Men and Foreign Men value rural first jobs, but have strong negative preferences for rural location in the long run. The coefficient on long-term salary is either positive or not statistically distinguishable from zero for all groups. This is in stark contrast to the negative coefficients on short-term salary for most demographic groups. This indicates that interns are willing to forgo some income during the residency period, while still valuing high earnings later on in their careers.

5.5 How much of FJEs are due to chance vs choice?

In this section, we try to quantify how much of the observed variation in FJEs is due to chance and how much of it is due to preference-driven choice. Throughout, the outcome of interest is doctor salaries ten years after the completion of medical school. We choose this outcome to enable comparisons with other studies on the persistence of labor market shocks.

To capture elements that are attributable to luck, we compare medical graduates that receive "good" lottery draws with those that receive "bad" lottery draws. The former would have had a large number of residency options to choose from, while the latter would have had fewer options. In expectation, the former would be able to choose residencies associated with higher doctor salaries in the long-run (i.e. ten years after medical school). This difference in access to high-return residencies can be attributed to luck, or a bad draw in the labor market.

To capture elements that are driven by resident preferences, we focus on how predicted choices differ from those that would maximize doctor salaries in the long-run. Predicted choices reflect the maximization of utility, not long-run income. We show that irrespective of the type of choice set that medical graduates are faced with, predicted choices consistently diverge from those that would maximize long-run salary. This suggests that at least some of the persistence that we and others (Oreopoulos *et al.* , 2012; von Wachter & Bender, 2006) document can be attributed to worker-driven responses.

Figure 5 presents these results graphically. Residency salary is placed on the vertical axis and lottery rank is placed on the horizontal axis, with higher ranks indicating worse lottery draws. The

[30] This is not an extreme assumption to make in the Norwegian context, since doctor salaries are observed publicly.

dashed line displays the residency salary for the hospital that would maximize long-term salary. Consistent with the fact that residency salary lowers long-run salary, the line is sloped upward for the first three quartiles. This indicates that graduates with larger choice sets are able to choose residencies associated with higher long-run salaries.

The red (undashed) line displays the residency salary for the hospital that maximizes worker utility. The fact that this line lies entirely below the dashed line indicates that residents are choosing to forego hospitals that would increase their salaries over the long-term. This is true even for those workers that face negative shocks (small choice sets) in the labor market. This indicates that at least some of the persistence of negative shocks is due to worker-driven preference response, since they could choose to avoid this outcome by gravitating towards hospitals associated with higher earnings in the long run.

6 Conclusion

This paper studies the impact of a graduate's first job on her career trajectory, and how job-seeking graduates' respond to these "first job effects" (FJEs). We exploit a natural experiment In Norway, where doctors' first jobs were allocated through a random serial dictatorship (RSD) mechanism until 2013.

Using administrative data on individual outcomes, we first confirm empirically that the residency allocation mechanism effectively randomized choice sets of hospitals across medical graduates. We exploit this variation in individual doctors' choice sets to estimate each type of job characteristic's impact on long-run earnings, place of residence, and specialization for each type of individual. We then account for these FJEs when estimating each worker type's preferences over job characteristics, enabling us to decompose the long-term consequences of a particular choice set into a component that is due to chance and a component that is due to workers' preference-driven response.

To our knowledge, these findings represent the first causal evidence on the impact of individual level variation in first jobs on individuals' long-term careers. The results indicate that early career shocks can have very persistent effects, even within highly specialized occupations.

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Graphs

FIGURE 1: NUMBER OF INDIVIDUALS AND HOSPITALS BY LOTTERY

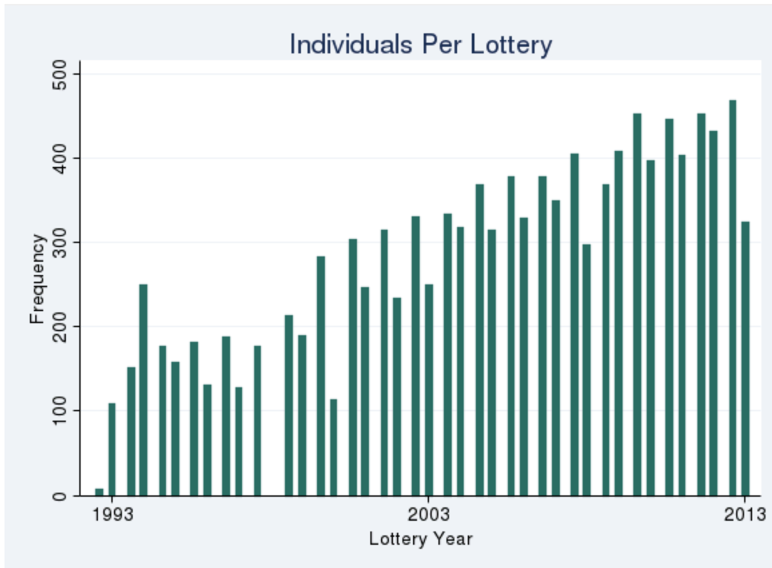


FIGURE 2: DOCTOR OBSERVATIONS POST-RESIDENCY

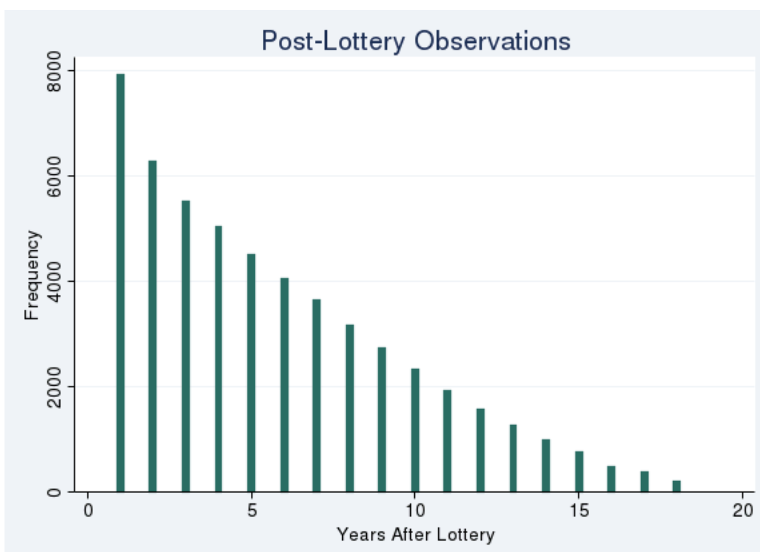
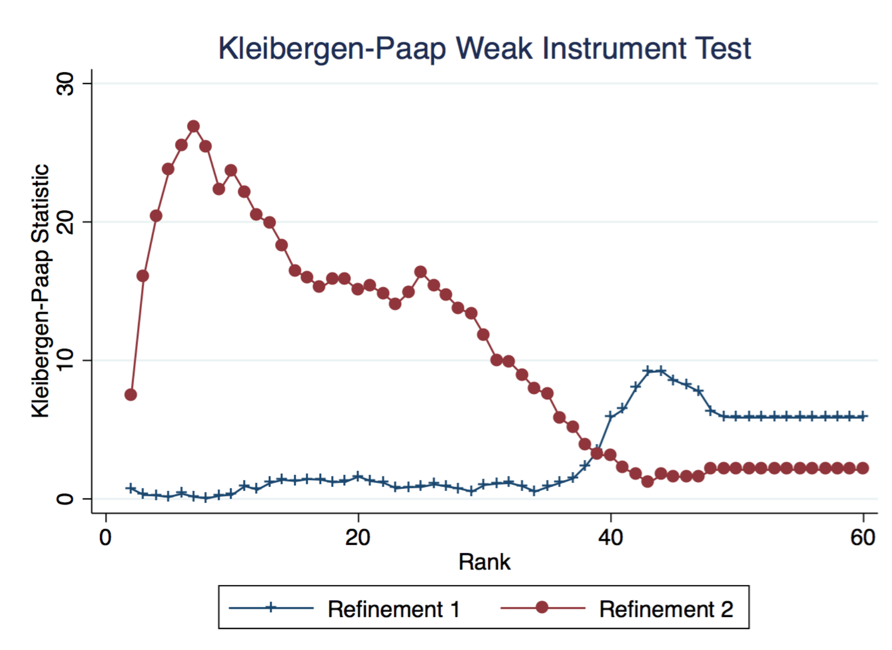


FIGURE 3: FIRST STAGE REFINEMENTS

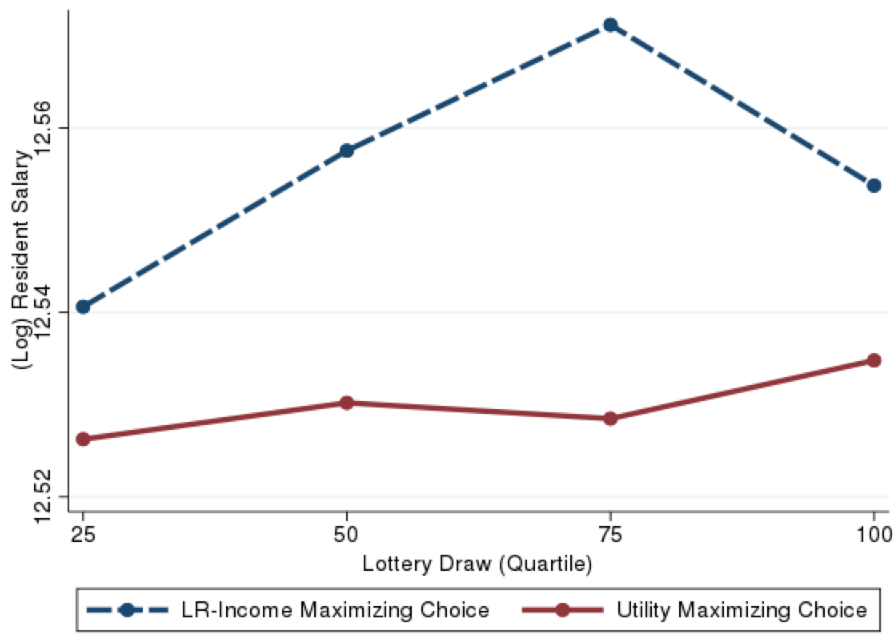


Notes: Mean and standard deviation as instruments

FIGURE 4: LONG-RUN EFFECTS

Notes: Separate regressions for each year post residency. χ^2 critical value with 8 degrees of freedom = 13.362 at the 10% level of significance (dashed line in graph above), 15.507 at the 5% level of significance.

FIGURE 5: DECOMPOSING FJES



Notes: The dashed line plots residency salary at the hospital that would maximize doctor salary 10 years later. The red line plots residency salary at the hospital that maximizes worker utility. The horizontal axis plots quartiles for lottery rank, with higher ranks indicating worse lottery draws and smaller choice sets.

Tables

TABLE 1: HOSPITAL CHARACTERISTICS

Firm Type	Health Clinics <10 Doctors	Hospitals $\geq$ 10 Doctors
Number of Doctors	1.79	69.35
Average Years of Experience	15.85	13.01
Number of Specialists	0.92	36.72
Rural Location	0.19	0.11
Average Hospital Salary	453.86	584.01
Average Total Salary	487.53	607.42
Average Doctor Income	562.82	642.46
Average Age	47.98	44.76
Proportion Male	0.74	0.62
Proportion Specialized	0.53	0.52
Proportion Married	0.68	0.65
Proportion Born Abroad	0.23	0.26
Specialists Present		
General Practice	0.35	0.81
Anesthiology	0.04	0.53
Occupational	0.06	0.21
Psychiatry	0.15	0.62
Paediatrics	0.03	0.4
Physiology	0.02	0.21
Gynaecology	0.03	0.45
Dermatology	0.01	0.17
Diagnostic	0.04	0.53
Surgery	0.07	0.6
Research	0.01	0.29
Internal Medicine	0.09	0.71
Public Health	0.11	0.35
Otolaryngology	0.03	0.27
Ophthalmology	0.04	0.25
Observations	34008	2632

Notes: Observations are at the hospital-year level; a hospital that is observed twice will count for two separate observations (since characteristics may change over time). Rural Location is defined as the proportion of population in the municipality that lives in rural areas. Hospital Salary, Total Salary and Income are in 1000 (Real) NOK. There are fewer than 34008/2632 observations for the following characteristics: Proportion Married, Born Abroad, Average Doctor Income, Salary, Average Age, Proportion Male, Rural Location

TABLE 2: DOCTOR CHARACTERISTICS

Characteristic	Total	Female	Male
Age	43.29	39.19	45.73
Cohabit	0.7	0.63	0.75
Male	0.63	0	1
Born Abroad	0.21	0.23	0.2
Number of Children	1	1.05	0.97
Specialists	0.48	0.34	0.56
Specialization			
General Practice	0.11	0.09	0.12
Anesthiology	0.06	0.04	0.07
Occupational	0.02	0.02	0.02
Psychiatry	0.1	0.13	0.08
Paediatrics	0.04	0.05	0.04
Physiology	0.01	0.01	0.01
Gynaecology	0.04	0.06	0.03
Dermatology	0.01	0.01	0.01
Diagnostic	0.08	0.08	0.08
Surgery	0.13	0.05	0.18
Research	0.01	0.01	0.02
Internal Medicine	0.16	0.13	0.18
Public Health	0.02	0.01	0.03
Otolaryngology	0.02	0.01	0.02
Ophthalmology	0.02	0.02	0.02
Income/Assets[1]			
Hospital Salary	518.9	449.17	560.39
Total Salary	549	466.34	598.2
Income	587	486.8	646.63
Business Income	37.99	20.46	48.42
Income After Tax	404.77	345.96	439.77
Deposits	198.44	170.44	215.1
Mutual Funds	35.77	22.7	43.55
Stocks Unregistered	137.52	98.29	160.87
Stocks Oslo SE	31.96	12.2	43.72
Real Estate	202.43	148.42	234.58
Debt	875.67	648.17	1011.07
Observations	214483	80025	134458

Notes: Observations are at the individual-year level; an individual that is observes twice will count as two separate observations. [1]Income/Assets are in multiples of 1000 Real NOK.

Table 3: Randomization Via Lottery

Individual Characteristic	ω_C	t-stat	R^2	N
Rurality of Age 15 Location	-0.011	-0.846	0.085	8267
Male	0.003	0.561	0.082	9828
Age	-0.001	-1.370	0.082	9828
Born Abroad	-0.005	-0.655	0.082	9828
Study Abroad	-0.0002	-0.021	0.095	2871

Notes: The dependent variable is the lottery draw number, normalized to lie between 0 and 1.

TABLE 4: PREFERENCES (MRS) FOR HOSPITAL CHARACTERISTICS: MALE X NORWEGIAN X RURAL

	Coefficient	p-value	Coefficient	p-value
	Rural Norwegian Male		**Rural Norwegian Female**	
ln(Hospital Salary)	-1.5696	0.0089	-1.2841	0.0069
Rural Location	0.9896	0.1557	0.6338	0.2668
Proportion Specialized	2.2436	0.0024	1.0784	0.0785
Internal Med. Spec	-0.0012	0.8452	-0.004	0.3634
Surgery Spec.	-0.0007	0.9113	-0.0001	0.9892
Diagnostic Med. Spec.	-0.0216	0.0067	-0.0048	0.4078
Psychiatry Spec.	0.0020	0.8820	-0.0062	0.569
Individuals	1375		1867	
Avg # of Alternatives	33		34	
	Urban Norwegian Male		**Urban Norwegian Female**	
ln(Hospital Salary)	-0.2249	0.6726	-1.2155	0.0372
Rural Location	0.0281	0.9691	-3.1776	0.0014
Proportion Specialized	2.4425	0.0008	0.9122	0.2445
Internal Med. Spec	-0.0187	0.0006	-0.0174	0.0013
Surgery Spec.	0.0165	0.0074	0.0107	0.0680
Diagnostic Med. Spec.	-0.0027	0.7027	0.0002	0.9748
Psychiatry Spec.	-0.0004	0.9744	0.0191	0.1143
Individuals	1381		1656	
Avg # of Alternatives	33		34	
	Foreign Male		**Foreign Female**	
ln(Hospital Salary)	1.4774	0.3307	-1.2088	0.2286
Rural Location	-6.4636	0.0602	-3.7478	0.0374
Proportion Specialized	3.4039	0.1108	0.1787	0.8972
Internal Med. Spec	-0.0473	0.0151	-0.0182	0.0389
Surgery Spec.	0.0387	0.0610	0.0155	0.1166
Diagnostic Med. Spec.	0.0480	0.0873	0.0079	0.5190
Psychiatry Spec.	0.0358	0.2791	0.0066	0.7363
Individuals	771		707	
Avg # of Alternatives	33		34	

TABLE 5: PREFERENCES (MRS) FOR LONG-TERM HOSPITAL CHARACTERISTICS

	Coefficient	p-value	Coefficient	p-value
	Rural Norwegian Male		**Rural Norwegian Female**	
ln(Long-Term Hospital Salary)	0.5163	0.5713	-1.0179	0.1418
Long-Term Rurality	4.8327	0.0201	-1.1661	0.4766
Long-Term Specialization	9.0542	0.0103	5.7596	0.0269
ln(Hospital Salary)	-1.8134	0.0083	-1.3349	0.0123
Rural Location	-0.8001	0.4764	1.1057	0.1891
Proportion Specialized	2.0324	0.0144	0.8268	0.2289
Internal Med. Spec	-0.0022	0.7228	-0.0019	0.6862
Surgery Spec.	0.0037	0.5912	-0.0005	0.9214
Diagnostic Med. Spec.	-0.0215	0.008	-0.0084	0.1555
Psychiatry Spec.	-0.0041	0.7823	-0.0172	0.1542
Individuals	1207		1695	
Avg # of Alternatives	33		34	
	Urban Norwegian Male		**Urban Norwegian Female**	
ln(Long-Term Hospital Salary)	-0.8236	0.3183	-1.1268	0.1464
Long-Term Rurality	-12.3673	0	-10.6118	0
Long-Term Specialization	-2.1823	0.4693	1.474	0.5778
ln(Hospital Salary)	-0.2561	0.672	-1.1581	0.0488
Rural Location	4.3765	0	1.3201	0.158
Proportion Specialized	2.4839	0.0034	0.5209	0.519
Internal Med. Spec	-0.0151	0.0066	-0.0142	0.0041
Surgery Spec.	0.0112	0.0811	0.0063	0.2529
Diagnostic Med. Spec.	-0.005	0.492	-0.0029	0.6549
Psychiatry Spec.	-0.0132	0.398	0.0113	0.346
Individuals	1236		1519	
Avg # of Alternatives	33		34	
	Foreign Male		**Foreign Female**	
ln(Long-Term Hospital Salary)	-2.7853	0.1788	1.3425	0.4081
Long-Term Rurality	-24.9915	0.0075	-12.7774	0.0084
Long-Term Specialization	-5.3944	0.499	1.1741	0.83
ln(Hospital Salary)	-0.3845	0.7987	-1.9329	0.1117
Rural Location	4.0115	0.1109	-0.1236	0.9502
Proportion Specialized	1.582	0.4376	-0.487	0.757
Internal Med. Spec	-0.0236	0.1138	-0.0208	0.0341
Surgery Spec.	0.0138	0.3956	0.0186	0.1074
Diagnostic Med. Spec.	0.0291	0.2022	0.0082	0.5404
Psychiatry Spec.	-0.0402	0.3199	0.0136	0.5249
Individuals	702		670	
Avg # of Alternatives	33		34	

Appendix

FIGURE A.1: LOTTERY PARTICIPANTS BY GENDER AND COUNTRY OF BIRTH

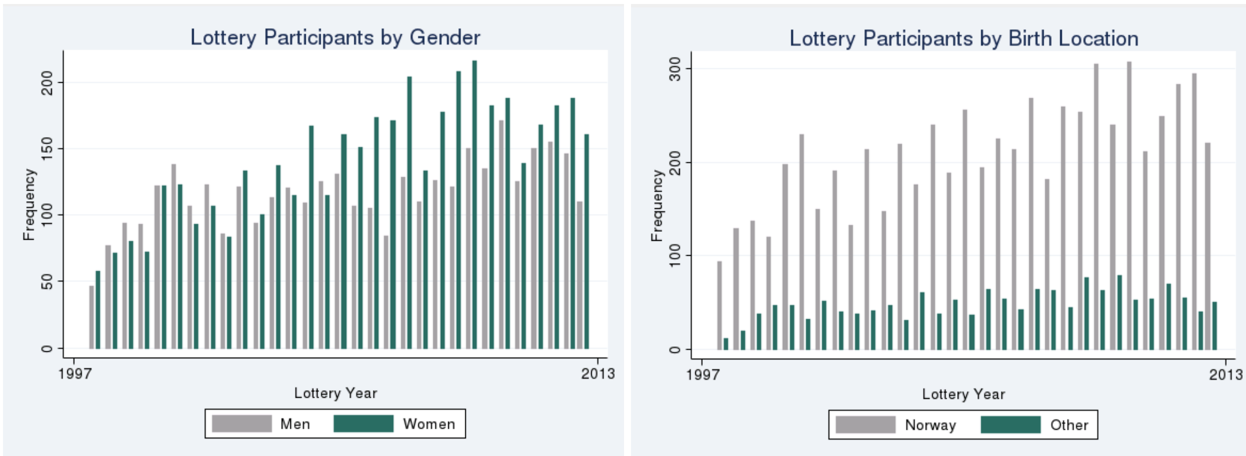
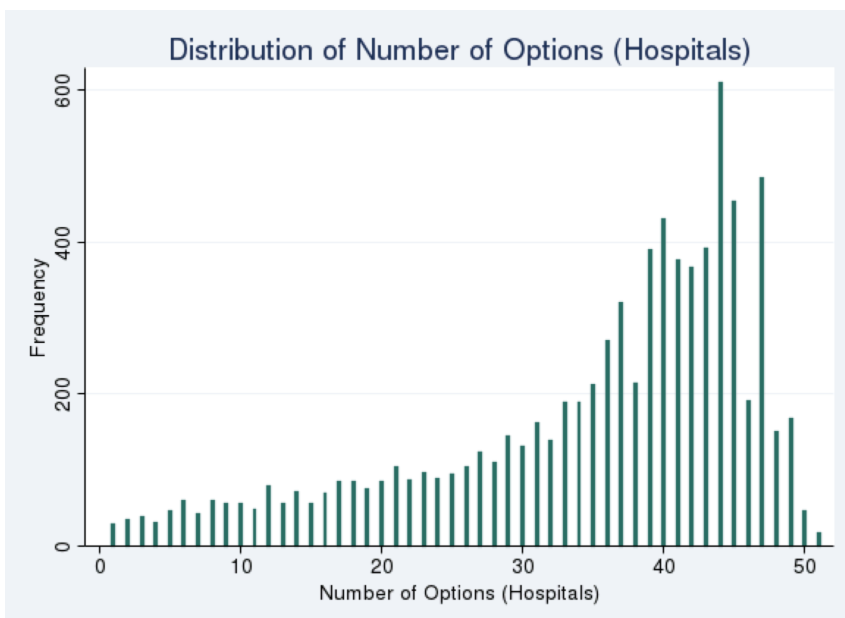
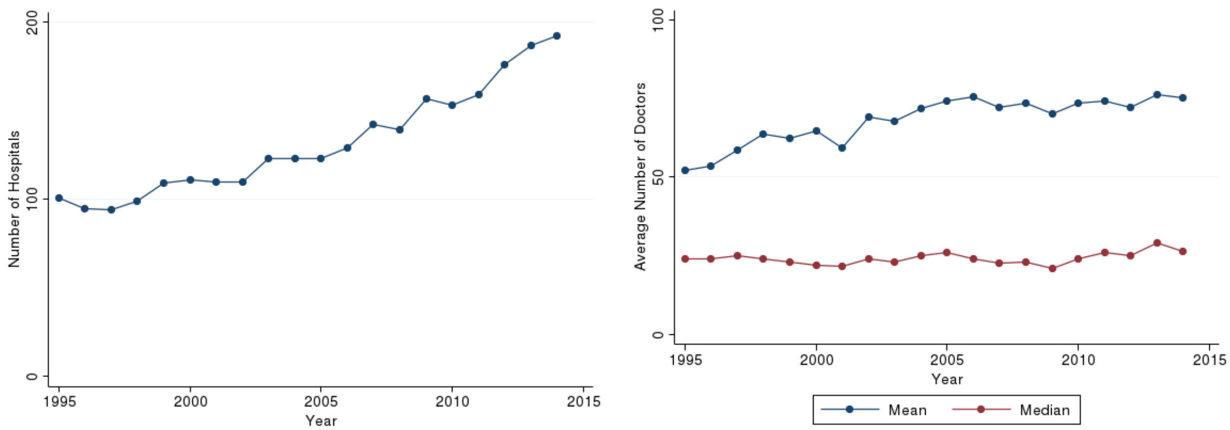


FIGURE A.2: NUMBER OF RESIDENCY OPTIONS FOR LOTTERY PARTICIPANTS



Notes: This graph displays the number of options available to medical graduates who participated in the lottery and chose a residency hospital.

FIGURE A.3: NUMBER OF HOSPITALS AND HOSPITAL SIZE OVER TIME



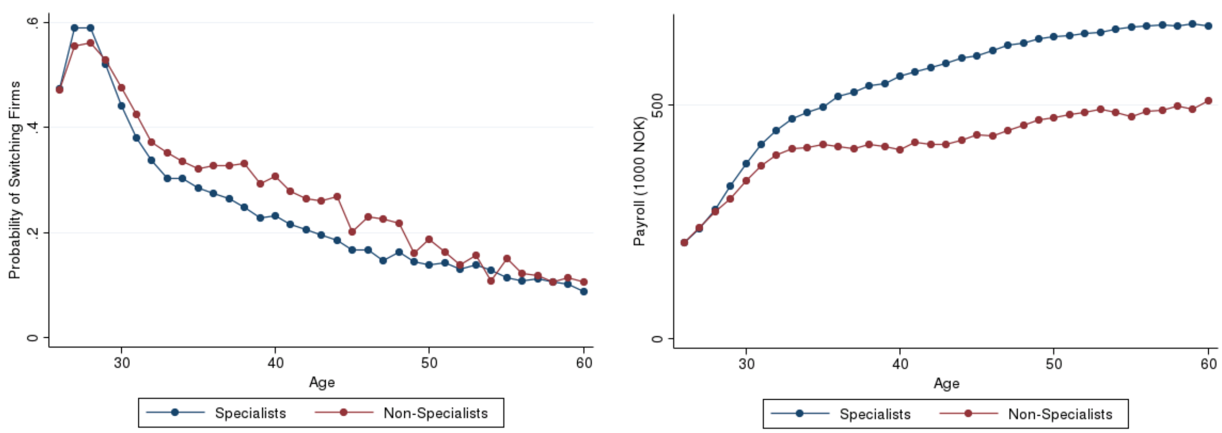
Notes: These graphs depict the number of hospitals and hospital size (number of doctors) over time.

TABLE A.1: CORRELATION ACROSS HOSPITAL CHARACTERISTICS

N = 2038	Number of Doctors	Avg. Years of Experience	Proportion Specialized	Rural Location	Hospital Salary	Total Income	Total Salary
Number of Doctors	1						
Avg. Years of Experience	0.01	1					
Proportion Specialized	0.03	0.65	1				
Rural Location	-0.19	-0.16	0.02	1			
Hospital Salary	0.07	0.19	0.39	0.21	1		
Total Income	0.01	0.23	0.38	0.21	0.79	1	
Total Total Salary	0.04	0.22	0.39	0.23	0.84	0.96	1

Notes: Observations are at the hospital-lottery level. If a hospital participates in two lotteries, it is counted twice (since characteristics may change over time). Rural Location is defined as the proportion of population in the municipality that lives in rural areas.Hospital Salary, Income and Total Salary are in 1000 (Real) NOK.

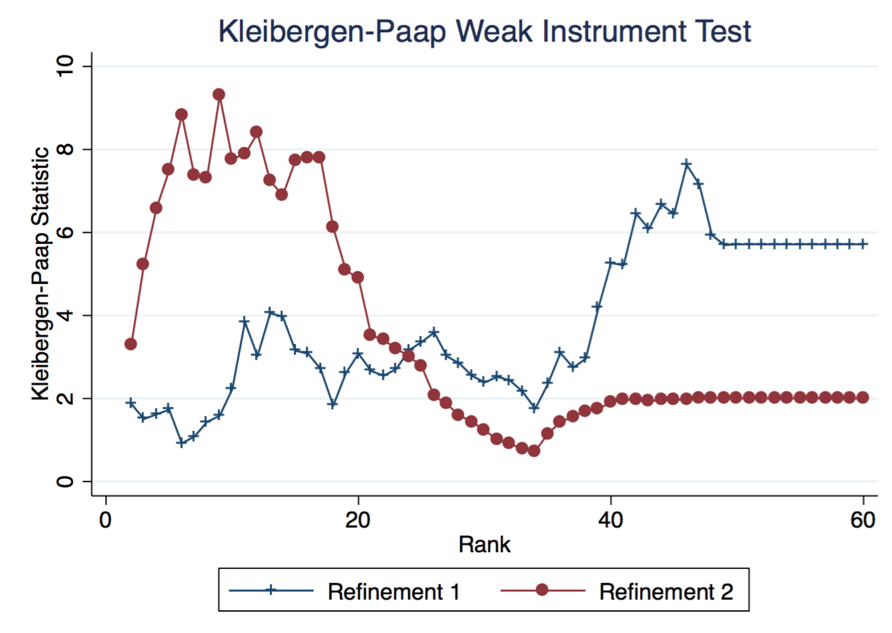
FIGURE A.4: RETURNS TO SPECIALIZATION



Notes: These graphs depict firm mobility and hospital salary amounts for doctors who specialize in their lifetime versus those who never do. Mobility is lower and hospital salary is higher for those who eventually specialize for most of their careers.

FIGURE A.5: ALTERNATIVE FIRST STAGE REFINEMENTS

(a) 25th, 50th and 75th Percentiles as Instruments



(b) Mean, 1st and 99th Percentiles as Instruments

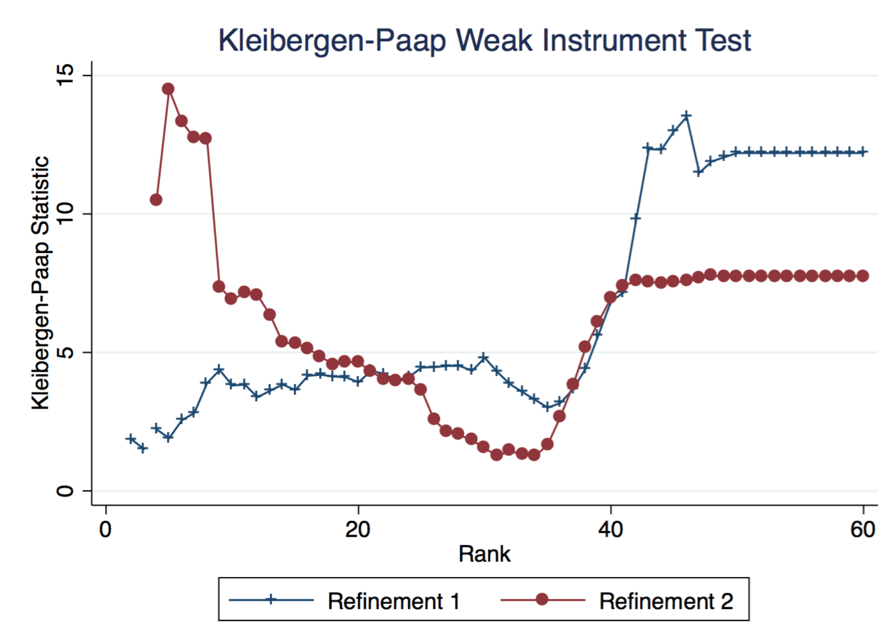
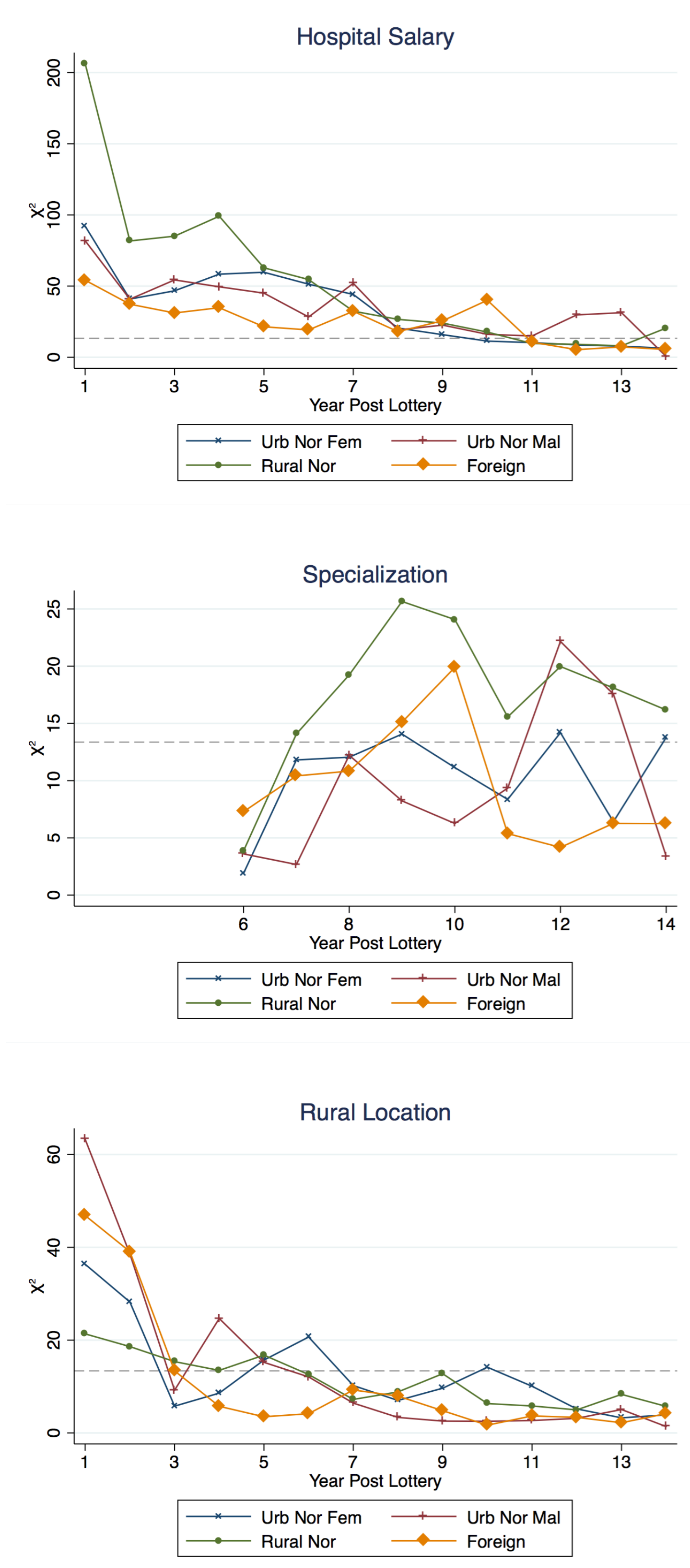


FIGURE A.6: LONG-RUN EFFECTS: DEMOGRAPHIC HETEROGENEITY



Notes: Separate regressions for each year post residency. χ^2 critical value with 8 degrees of freedom = 13.362 at the 10% level of significance (dashed line in graph above), 15.507 at the 5% level of significance.

TABLE A.2: SALARY VARIATION WITH FIRM AND INDIVIDUAL CHARACTERISTICS

Characteristic	Coefficient	Coefficient
Doctors 2-3	70.46	72.64
	(2.94)	(2.88)
Doctors 4-9	114.13	106.43
	(2.6)	(2.61)
Doctors ≥ 10	184.84	139.54
	(2.06)	(2.16)
Average Years of Experience	-3.56	-4.04
	(0.15)	(0.15)
Proportion Specialized	49.62	101.9
	(3.66)	(3.46)
Rural Location	5.13	44.43
	(3.58)	(3.56)
Male	69.4	60.71
	(0.94)	(0.93)
Born Abroad	-0.88	-9.76
	(1.14)	(1.11)
Specialist	107.18	
	(1.44)	
General Practice		-57.98
		(1.68)
Anesthiology		151.17
		(2.31)
Occupational		38.47
		(3.25)
Psychiatry		64.87
		(1.64)
Paediatrics		65.5
		(2.17)
Physiology		8.59
		(3.68)
Gynaecology		118.7
		(2.45)
Dermatology		26.76
		(5.75)
Diagnostic		113.35
		(1.99)
Surgery		152.85
		(1.67)
Research		27.61
		(4.16)
Internal Medicine		48.33
		(1.34)
Public Health		9.49
		(2.91)
Otolaryngology		70.98
		(3.44)
Ophthalmology		51.98
		(3.29)
Observations	207092	207092
R^2	0.39	0.42
Time F.E.	×	×
Age F.E.	×	×

Notes: Robust standard errors in parentheses.

TABLE A.3: PREFERENCES FOR HOSPITAL CHARACTERISTICS: MALE X NORWEGIAN X RURAL

	Coefficient	p-value	Coefficient	p-value
	Rural Norwegian Male		**Rural Norwegian Female**	
Hospital Salary	-0.5636	0.0046	-0.5065	0.0045
Rural Location	0.3553	0.1830	0.2500	0.2885
Proportion Specialized	0.8057	0.0032	0.4254	0.0851
Internal Med. Spec	-0.0004	0.8459	-0.0016	0.3698
Surgery Spec.	-0.0003	0.9114	0.0000	0.9892
Diagnostic Med. Spec.	-0.0077	0.0059	-0.0019	0.4104
Psychiatry Spec.	0.0007	0.8824	-0.0024	0.5632
log(Num. of Doctors)	0.3591	0.0000	0.3944	0.0000
Individuals	1375		1867	
Avg # of Alternatives	33		34	
	Urban Norwegian Male		**Urban Norwegian Female**	
Hospital Salary	-0.0856	0.6714	-0.4148	0.0295
Rural Location	0.0107	0.9692	-1.0844	0.0000
Proportion Specialized	0.9303	0.0011	0.3113	0.2508
Internal Med. Spec	-0.0071	0.0006	-0.0059	0.0014
Surgery Spec.	0.0063	0.0035	0.0037	0.0577
Diagnostic Med. Spec.	-0.0010	0.7039	0.0001	0.9748
Psychiatry Spec.	-0.0002	0.9744	0.0065	0.1263
log(Num. of Doctors)	0.3809	0.0000	0.3413	0.0000
Individuals	1381		1656	
Avg # of Alternatives	33		34	
	Foreign Male		**Foreign Female**	
Hospital Salary	0.2605	0.3222	-0.3686	0.2085
Rural Location	-1.1398	0.0019	-1.1427	0.0048
Proportion Specialized	0.6002	0.0981	0.0545	0.8976
Internal Med. Spec	-0.0083	0.0026	-0.0055	0.0380
Surgery Spec.	0.0068	0.0153	0.0047	0.0878
Diagnostic Med. Spec.	0.0085	0.0329	0.0024	0.5093
Psychiatry Spec.	0.0063	0.2884	0.0020	0.7398
log(Num. of Doctors)	0.1763	0.0015	0.3049	0.0000
Individuals	771		707	
Avg # of Alternatives	33		34	

TABLE A.4: LONG-TERM HOSPITAL SALARY

Year	<2	2-3	4-5	6-7	8-9	10-11	12-13	14-15
Residency Salary	0.021	-0.027	0.036	-0.136	-0.185	-0.257	-0.328	-0.677
	(0.036)	(0.063)	(0.050)	(0.074)	(0.084)	(0.106)	(0.135)	(0.267)
Residency Rurality	-0.027	-0.767	1.227	2.795	0.232	3.495	0.603	4.858
	(0.629)	(1.370)	(1.335)	(1.436)	(1.398)	(1.877)	(2.291)	(4.989)
Residency Proportion	0.153	2.803	0.196	1.620	2.619	1.928	3.198	5.482
of Specialists	(0.549)	(0.818)	(0.712)	(0.889)	(1.181)	(1.463)	(1.695)	(3.206)
Residency Hospital Size	0.019	-0.216	-0.379	-1E-4	0.269	0.442	0.633	1.914
	(0.094)	(0.176)	(0.199)	(0.240)	(0.271)	(0.334)	(0.475)	(0.947)
Observations	15,280	11,681	9,450	7,617	5,836	4,190	2,802	1,707
Mean	2.999	4.794	6.121	6.537	7.132	7.791	8.543	9.083

TABLE A.5: LONG-TERMSPECIALIZATION

Year	<2	2-3	4-5	6-7	8-9	10-11	12-13	14-15
Residency Salary	4E-5	-1E-5	-0.001	1E-4	-0.021	-0.043	-0.037	-0.021
	(0)	(0.001)	(0.001)	(0.006)	(0.013)	(0.018)	(0.023)	(0.028)
Residency Rurality	-0.004	-0.009	0.028	-0.216	-0.375	0.243	-0.008	-0.021
	(0.002)	(0.012)	(0.035)	(0.119)	(0.240)	(0.427)	(0.454)	(0.584)
Residency Proportion	4E-4	-0.005	0.028	0.194	0.577	0.599	0.436	0.338
of Specialists	(0.002)	(0.008)	(0.022)	(0.086)	(0.212)	(0.246)	(0.288)	(0.344)
Residency Hospital Size	1E-4	0.001	-0.002	-0.031	-0.018	0.074	0.075	0.0128
	(0.001)	(0.002)	(0.006)	(0.019)	(0.046)	(0.056)	(0.080)	(0.102)
Observations	15,280	11,681	9,450	7,617	5,836	4,190	2,802	1,707
Mean	0.001	0.002	0.004	0.051	0.254	0.488	0.671	0.778

TABLE A.6: LONG-TERM RURAL LOCATION

Year	<2	2-3	4-5	6-7	8-9	10-11	12-13	14-15
Residency Salary	-0.028	-0.001	-0.016	-0.011	-0.007	-0.007	-0.008	-0.010
	(0.005)	(0.009)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)
Residency Rurality	0.907	0.453	0.461	0.317	0.228	0.355	0.293	0.752
	(0.115)	(0.148)	(0.111)	(0.082)	(0.080)	(0.096)	(0.110)	(0.256)
Residency Proportion	-0.223	-0.265	-0.110	-0.096	-0.101	-0.176	-0.136	-0.253
of Specialists	(0.090)	(0.123)	(0.049)	(0.060)	(0.057)	(0.061)	(0.062)	(0.109)
Residency Hospital Size	0.063	0.015	0.050	0.034	0.019	0.013	0.011	-0.020
	(0.014)	(0.020)	(0.012)	(0.018)	(0.018)	(0.014)	(0.018)	(0.033)
Observations	15,207	11,638	9,414	7,592	5,820	4,177	2,797	1,707
Mean	0.135	0.142	0.098	0.090	0.085	0.080	0.079	0.086

Chapter 3

Election by Community Consensus:
Effects on Political Selection and Governance*

Ashna Arora†

Abstract

This paper evaluates the effects of encouraging the selection of local politicians in India via community-based consensus, as opposed to a secret ballot election. While secret ballot elections prevent vote capture by guaranteeing voter anonymity, consensus-based elections may improve welfare by promoting the exchange of information. I find that incentives for consensus-based elections crowd in politicians that are younger and more educated, but lead to worse governance as measured by a fall in local expenditure and regressive targeting of workfare employment. These results are consistent with qualitative evidence that finds that community-based processes are prone to capture by the local elite, and need not improve the quality of elected politicians or governance.

*I am deeply grateful to Francois Gerard, Suresh Naidu and Bernard Salanié for guidance and support. For helpful discussions, I would like to thank Siddharth Hari, Nandita Krishnaswamy, Daniel Rappoport, Christoph Rothe and numerous participants at the Applied Microeconomics and Development Colloquia at Columbia University as well as the Conference on Economic Growth and Development at the Indian Statistical Institute. I also thank Kunjal Desai for help with data access. This research was supported by the Wueller Pre-Dissertation Award at Columbia University. All errors are my own.

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1 Introduction

Since the 1990s, developing countries have undertaken a broad range of decentralization reforms, aimed at enhancing the role of local stakeholders in policy making and program implementation (Bardhan 2002). These reforms have led to the creation of democratic governments at the local level, as well as the devolution of authority to existing local governments. In the last decade, research and policy attention has shifted to deepening democratic processes within these institutions by limiting the influence of elites and enhancing community participation in local decision making. These policies include the use of community monitoring (Olken 2007), community meetings (Besley *et al.* 2005, Rao & Ibáñez 2005, Alatas *et al.* 2013) and direct democracy (Olken 2008, Beath *et al.* 2013, Hinnerich & Pettersson-Lidbom 2014) to implement programs based on the consensus of local stakeholders.

This paper examines the effects of promoting community consensus as a tool to select political representatives themselves, rather than as a tool to monitor or alter policy decisions after politicians have assumed office. I first test for changes in observable politician characteristics, such as age and education, to understand whether the policy hurts incumbents and to quantify its impact on political selection. I also examine changes in measurable aspects of governance at the village level, such as the amount of local expenditure and targeting of workfare employment. These outcomes are directly affected by local politicians and can, therefore, be used to estimate the impact of the policy on governance.

I find that promoting consensus-based elections significantly influence who communities elect, and their performance once in office. Incentives for consensus-based elections crowd younger, more educated representatives into political office. However, these elections also lead to a reduction in government size, reflected in a reduction in total expenditure, and more regressive targeting of workfare employment by the local council.[1] These findings are indicative of worse governance, since development expenditure and workfare employment disproportionately benefit the poorest households in Indian villages (Imbert & Papp 2015). Overall, the results are consistent

[1] Regressivity is measured by the proportion of workfare employment allocated to historically marginalized sections of the population.

with qualitative evidence that finds that community-based processes in general, and consensus-based elections in specific, are prone to elite capture and can lead to worse governance.

To estimate the impact of encouraging consensus-based elections, I compile a new dataset containing detailed information on candidates, politicians and governance indicators at the village level in Gujarat, a state in Western India, for the years 2011-15. While many states in India incentivize consensus-based elections at the village level, Gujarat offers untied financial grants that increase discontinuously with village population. In the 2011 elections in Gujarat, villages with populations greater than 5000 faced substantially larger incentives for elections via community consensus - the financial grant increased by fifty percent at the threshold, from 13 to 20 per cent of the median village budget. This increase in financial incentives is used to set up a regression discontinuity design, which tests for the causal impact of incentivizing consensus elections on political selection and governance. The identifying assumption is that unobservables vary smoothly around this population threshold.

This setting also allows me to circumvent the contamination of estimates by multiple treatments, a common drawback of regression discontinuity designs. Two features of the local political system increase at the population threshold of 5000 - the consensus election grant and the number of political representatives. However, the number of political representatives also increases discontinuously at population thresholds other than 5000. Estimates at these alternative thresholds are used to show that this contaminating treatment (increase in the number of council members) does not drive the findings on electoral competition, politician identity and governance.

Why would crowding in younger, more educated politicians worsen governance? The state government does not place any restrictions on how village communities reach a consensus about their political representatives. Survey evidence indicates that local elites usually nominate candidates and mobilize support for their election by consensus, i.e., without formal opposition (Breman 2011). If these candidates are inexperienced and merely serve as political placeholders, they may lack both the ability and motivation to undertake administrative and development expenditures within the village, and negotiate with bureaucrats outside the village to influence funding towards workfare employment.[2] Financial incentives for consensus-based elections could, therefore, worsen governance by crowding in politicians that rely on the support of a handful of local elites instead of all village residents.

This paper contributes to the growing literature on the impact of electoral institutions on political selection and governance outcomes (Diermeier *et al.* 2005, Keane & Merlo 2010, Banerjee *et al.* 2011, Banerjee *et al.* 2017). The results are also consistent with theoretical work that shows that political competition and community participation may have negative or positive effects (Khwaja 2004, Caselli & Morelli 2004, Lizzeri & Persico 2005, Mattozzi & Merlo 2008). Empirical work shows that reducing political competition can worsen legislator quality and performance (Brazil, Ferraz & Finan 2009) and is associated with anti-growth policies (United States, Besley *et al.* 2010). This paper finds broadly similar results in the Indian village setting - incentives for consensus-based elections lower competition and crowd in younger (albeit more educated) politicians, and reduce expenditure and worsen employment targeting by the local government.

Additionally, I find support for the citizen-candidate models of Osborne & Slivinski (1996) and Besley & Coate (1997), which highlight the influence of politician identity on governance outcomes. My results add to the extensive literature documenting the influence of visible politician characteristics on governance outcomes in India (Pande 2003, Chattopadhyay & Duflo 2004, Rajaraman & Gupta 2012, Afridi *et al.* 2013) and other countries (Powley 2007, Washington 2008).

This paper also contributes to the debate on the effects of elite influence on governance and social welfare. Studies show that elite capture can have sizable negative consequences in some contexts (Besley *et al.* 2004, Acemoglu & Robinson 2008, Caeyers & Dercon 2012, Acemoglu *et al.* 2014), but that these effects may be small or completely absent in other settings (Bardhan & Mookherjee 2006, Alatas *et al.* 2013, Beath *et al.* 2013).[3] This paper concurs with the findings of the former set of papers by showing that at least in the short term, elite influence in elections can influence politician identity and substantially worsen governance.

Finally, these results add to the burgeoning literature on regression-discontinuity designs in

[2] Political and administrative inexperience has been shown to be an important determinant of implementation inefficiencies and leakages in the Indian village setting (Afridi *et al.* 2013)

[3] Baland & Robinson (2012) show that the introduction of the secret ballot reduced elite influence over voting decisions, but do not measure its impact on government performance.

political economics (Lee 2008, Ferraz & Finan 2009, Pettersson-Lidbom 2012, Hinnerich & Pettersson-Lidbom 2014).

The remainder of this paper is organized into four sections. Section 2 describes the institutional setting and the data sources. Section 3 details the empirical strategy and Section 4 presents the results. Section 5 concludes.

2 Setting and Data

This section provides detailed information about the functioning of village governments in Gujarat, the implementation of the Samras (consensus) Panchayat scheme, as well as the datasets used in the empirical analysis.

Institutional Background

The Seventy Third Amendment to the Indian Constitution in 1992 mandated the creation of a three tiered local government system, at the district, block and village level (in descending order of size of jurisdiction) across states in India. This study focuses on elected councils at the village level, also called Gram Panchayats (henceforth, GPs) in Gujarat. GP members are directly elected for five-year terms by village residents and elections are not fought on party lines, i.e. candidates are not affiliated with political parties at the state or national level. The jurisdiction of each GP is divided into a number of mutually exclusive wards, and efforts are undertaken to ensure that each ward contains the same number of residents. The population of each ward then elects a single representative to occupy a GP seat. In Gujarat, the number of ward members is fixed at 7 for GPs with populations up to 3000, and increases by 2 for every multiple of 1000 thereafter. Figure 1 uses electoral data to plot the actual number of GP members elected in the 2011 elections against GP population in Gujarat, and shows that this rule was closely followed in practice. The village community as a whole also directly elects the president of the GP.

The Seventy Third Constitutional Amendment also mandated reservations for women, and three disadvantaged classes - Scheduled Castes, Scheduled Tribes and Other Backward Classes at the district, block and village level.[4] At least 33 per cent of president and council member seats are reserved for women. Panel B of Figure 1 plots the number of seats reserved for women against GP population. The number of seats reserved for women increases discontinuously at each population threshold except the thresholds 5000 and 8000. For Schedules Castes, Scheduled Tribes and Other Backward Classes (henceforth, SC, ST and OBC respectively) the proportion of reserved seats is mandated to be as close as possible to their respective population shares in the state. Figure A.1 plots seats reserved for each of these three categories against GP population. While there is a distinct jump in the number of OBC seats at the population threshold 6000, no other visible discontinuities are seen at the other population thresholds.

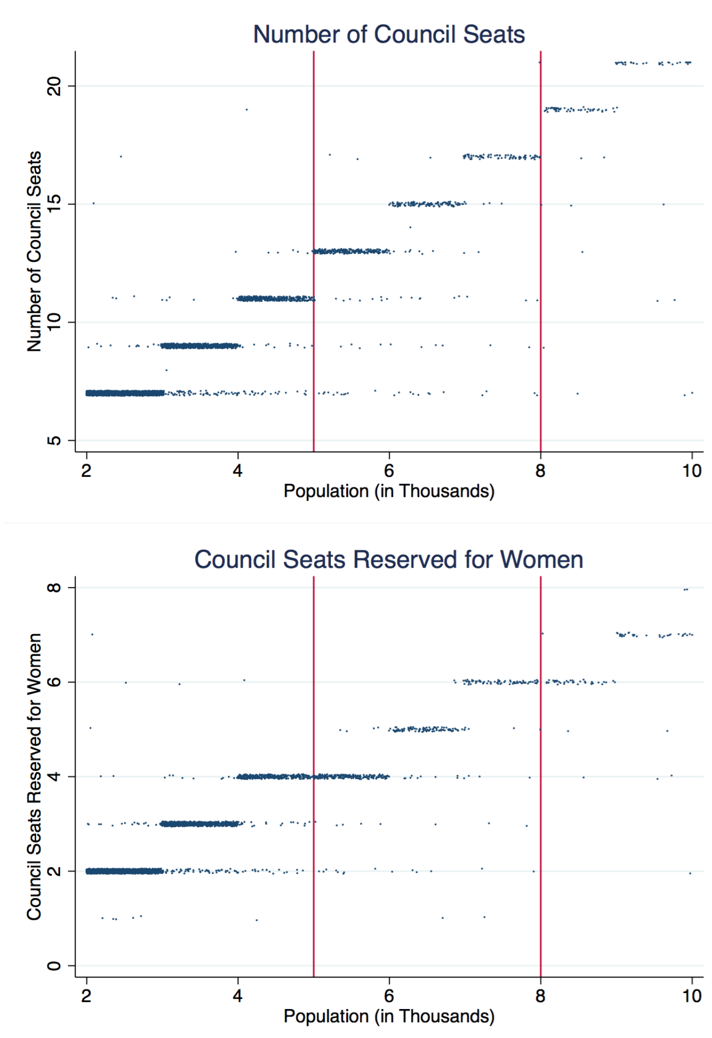
GP functions include income generation via tax collection, the upkeep of local public goods, and the implementation of various development programs. A sizable proportion of GP revenue comes in the form of grants from central and state governments, but GPs collect a variety of taxes and fees within their jurisdictions. These include water, property and trade taxes, and to a lesser extent revenue from fees, cesses and rental income. The GP allocates its budget to administrative expenses like salaries, the provision and maintenance of various local public goods such as roads and irrigation canals, as well as the upkeep of services like sanitation at the village level. GPs are also required to organize and preside over two town-hall style meetings called Gram Sabhas every year. While all village residents are invited to attend these meetings, in practice attendance varies considerably across GPs.

GPs are also responsible for implementing social welfare programs like the National Rural Employment Guarantee Act (henceforth, NREGA). NREGA is funded by the central government, and guarantees one hundred days of employment a year to each rural household. Whether this guarantee is met in practice depends to a large extent on the elected council, since they are responsible for aggregating local preferences and filing requests for NREGA funds at the block level.[5] Once the project has been sanctioned, GPs exert considerable influence in the targeting of program funds, since they are responsible for enlisting program beneficiaries. Therefore, sec-

[4]The system of rotating, randomized reservations creates exogenous variation in politician identity, and has lead to a large literature linking politician identity to policy outcomes such as public good provision (Chattopadhyay & Duflo (2004), Rajaraman & Gupta (2012), Dunning & Nilekani (2013)).

[5]For instance, many GPs have zero person-days of employment generated under NREGA.

FIGURE 1: NUMBER OF SEATS INCREASE WITH GP POPULATION



Notes: GP Seats reserved for women do not increase at the population thresholds 5000 and 8000.

tion 4 examines the impact of incentivizing consensus-based elections on both the overall level of employment generation, as well as who this employment is targeted towards.[6]

[6]NREGA employment data is reported by block and village level authorities, not directly by employment beneficiaries. Therefore, the estimates presented in this paper may be driven by changes in reporting behavior, not changes in actual employment. However, Section 4 shows that targeting, not creation, of NREGA employment is the dimension that is most affected by the change in political representation. As long as the cost of misreporting NREGA employment is similar across demographic groups, it is likely that these estimates reflect more regressive targeting.

Consensus-Based Elections

Financial incentives that encourage elections via community consensus have been offered by many state governments in India, for differing periods of time. For instance, Andhra Pradesh has offered financial incentives since 1964, while more recent implementers include Punjab and Haryana, who first offered incentives in 2008 and 2010 respectively.

This study focuses on Gujarat, a state in Western India, for three reasons. One, the incentive amount increases sharply at a fixed population threshold. This is not the case in states like Himachal Pradesh and Punjab. Two, the distribution of villages around the population threshold 5000 is dense enough to be able to conduct the empirical analysis.[7] This is not the case in Andhra Pradesh, for instance, where the incentive amount increases discontinuously if population exceeds 15,000; the population distribution around the cutoff point 15,000 is extremely sparse - only 64 GPs lie within the population range 14,000 to 16,000.[8] Third, the scheme has been implemented in Gujarat since 2001, so it is fair to assume that village residents understand its functioning, and believe that the government will pay out the promised grant amounts. Credibility has been a problem with recent implementers such as Punjab and Haryana, who failed to pay out the grants after the 2010 elections.[9]

The Gujarat Panchayat, Rural Housing and Rural Development Department is the agency that provides financial incentives for elections based on community consensus. The scheme's stated objectives are to promote social cohesion by minimizing electoral conflicts, and to reduce electoral expenses for candidates as well as the Gujarat State Election Commission (henceforth, SEC). The Gujarat SEC benefits financially if villages are able to agree upon a single candidate for each GP seat. This is because an unopposed candidate for a political post eliminates the need to set up polling booths and hire the associated electoral personnel. These expenses are described in detail below.

[7] Figure A.2 plots the distribution of GP population based on the 2001 Census.

[8] This estimate is based on village level population and GP Names provided in the 2011 Census.

[9] The state government is legally obligated to pay these amounts. See http://indianexpress.com/article/india/india-others/hc-rap-for-govt-for-failure-to-pay-panchayat-incentive for an instance where a legal case was filed against the Punjab government. As this case demonstrates, it may take many years for a legal case to be processed in court, and even more time before the state government complies with the court's orders.

The Gujarat Panchayati Raj Act (1994) provides extensive details on how GP elections are to be conducted. First, the Gujarat SEC notifies the GP about which seats are reserved for women, SCs and STs. An individual can contest the election if he or she belongs to the reserved category, or if the seat is unreserved. Interested candidates are invited to file nomination papers within a few days of the initial announcement. All nominations are scrutinized to ensure that they satisfy the eligibility criteria, which vary by state. In Gujarat, candidates below the age of 21, or those who are not registered as voters, cannot stand for election in GPs. Candidates have a few days to appeal against the rejection of their nomination papers, as well as run election campaigns. At the end of this period, polling booths are set up within the GP. The day of polling is usually declared as a local holiday. Every individual above the age of 18 who is registered as a voter is eligible to vote in GP elections. Efforts are made to count votes on the same day as polling. Electoral personnel must be hired to ensure free and fair polls, which can include the scrutiny of nomination papers and election expenditure, detection and prevention of voter impersonation, maintenance of voting secrecy, scrutiny of doubtful/invalid votes, supervision of counting and recounting, as well as the declaration of final vote shares.

Samras (Consensus) Panchayat in Gujarat

Since 2001, Gujarat has incentivized the election of GP members via public consensus under its Samras (consensus) Panchayat scheme. Village residents are encouraged to deliberate amongst themselves, and reach a consensus on who their political representatives should be. This scheme is aimed at preventing multiple candidates from standing for election, so that the sole candidate to file nomination papers can be declared as the unopposed winner. This prevents the need to organize official elections, reducing the state government's expenditure on the set up of polling booths and the hiring of election officers. The policy has been fairly successful. In the 2011 elections, one out of every seven GPs in Gujarat were elected by "consensus".[10] This means that each council seat in these GPs was filled by someone who faced no formal opposition.

The state government encourages consensus-based elections by providing untied grants[11] to

[10]There were over 13,000 GPs in Gujarat in 2011 - just under 2,000 were elected without opposition.

[11]The state government also provides unguaranteed benefits such as informal priority in project approval and imple-

TABLE 1: SAMRAS INCENTIVE AMOUNTS BY GP TYPE

Elected by "Consensus" for the	First Time		Second Time		Third Time	
Gender Composition	Men & Women	All-Women	Men & Women	All-Women	Men & Women	All-Women
Population ≤ 5000	200	300	250	375	312.5	468.75
Population > 5000	300	500	375	625	468.75	781.25

Notes: Amounts displayed are in INR 1000, which is approximately $15 ($45 in PPP terms). "Consensus" indicates that none of the GP members faced any formal political opposition.

councils elected without formal opposition, i.e. it directly rewards politicians who ensure that no other candidates stand for election. This grant increases discontinuously with population, is higher if an all-women council is chosen, and increases if the council is chosen without opposition for the second or third time.[12] Table 1 displays the grant amounts for each of these categories, which increase discontinuously as population exceeds 5000 irrespective of the composition of the council. This discontinuity in financial incentives is exploited to set up a regression discontinuity design in Section 3. The grant amount is paid only if each and every GP member is elected without formal opposition. This means that, on average, a village community must agree upon eight individuals as ward members and a President, and ensure that these are the only candidates to file official nomination papers and stand for election.

The state government does not delineate formal procedures or place any restrictions on how village residents should reach a consensus about their political representatives. Naturally, instances of creative approaches to reach a consensus abound. For instance, the village Kumkuva in south Gujarat organized a private election to choose amongst three competing candidates and ensure the receipt of the financial grant.[13] The village Vadavali, home to a substantial number of

mentation (Guha 2014, https://planning.gujarat.gov.in/images/pdf/Sectorial-Profile-NEW-2015-16.pdf) and the ability to influence taluka and district planning processes (https://medium.com/nakabandi/samras-gram-incentivising-your-way-to-consensus-in-gujarat-71f32d3514f8). The government also provides extra incentives for those opting for samras consecutively for the third time: (a) schools (up to grade eight); (b) solar street lights; (c) pucca roads (Bandi 2013).

[12]The grant amounts for consensus elections were first introduced in 2001. Therefore, it is not possible for any given GP to be elected by consensus for a fourth time.

[13]See http://deshgujarat.com/2011/12/12/a-remote-village-does-something-that-neither-modi-nor-the-election-commission-might-have-thought-of/

Hindu and Muslim families, has decided to divide the President's five year term equally between a Hindu and Muslim President (two and a half years each).[14] However, survey evidence suggests that it is usually local elites who nominate candidates and mobilize consensus-based support to ensure receipt of the monetary benefits (Breman 2011, Bandi 2013, Ganguly 2013, Guha 2014).

The political economy literature has consistently documented the substantial authority that local elites exert over decision making at the community level (Olken 2007, Alatas *et al.* 2013, Acemoglu *et al.* 2014). It is, therefore, unsurprising that village elders and landowning caste members are reported to be heavily involved in nominating political candidates and mobilizing consensus-based support for them. For instance, the dominant[15] Rajput residents of Gopalpura GP nominated women belonging to SC and ST groups for election by consensus in 2011.[16] This anecdote is consistent with Breman (2011), who describes the process of nominating candidates for consensus based elections in four Gujarati villages Gandevigam, Chikhligam, Bardoligam and Atulgam:

" The dominant caste-class of landowners state in the village assembly (held twice a year) who are going to be the next sirpanch (President) and members of the village council. It is possible to turn down the invitation to be nominated ... but alternative names cannot come up in the hearing."

Breman (2011) further describes an underwhelming approach to governance by councils so-elected:

"In our observation none of the councils in Gandevigam, Chikhligam, Bardoligam or Atulgam has a record of activity to show that village democracy is indeed practiced. The members are not involved in the

[14] This decision was made at a town-hall style meeting (gram sabha) in which leaders of all communities participated. See http://www.ndtv.com/india-news/day-before-riot-gujarat-village-split-sarpanchs-term-for-muslim-hindu-1674486 for more details.

[15] Dominant in terms of population share and socioeconomic status.

[16] The village has had official elections only thrice after Independence. There are about 290 Rajput families, 125 ST families and 65 SC families in the village. "During a village meeting, village elders and women suggested that a chance should be given to the women of SC and ST families as it would bring a lot of harmony among the villagers. The suggestion was readily accepted." For further details, see http://archive.indianexpress.com/news/narmada-s-rajput-village-appoints-tribal-woman-its-sarpanch/889943/

handling of local governance, there is no schedule for meetings and business is attended to by the talati, in charge of administration, and the sirpanch. The latter may be a figurehead only ... where the exercise of power is firmly in the hands of members (sic) who belong to the dominant caste-class of landowners ... "

These anecdotes do not imply that the rural poor have no space left for assertion, or that political representatives elected by community consensus cannot increase access to development programs and improve public good availability. However, political figureheads may lack the willingness to overcome the drawbacks of political and administrative inexperience, a significant determinant of implementation inefficiencies in the Indian village setting (Afridi *et al.* 2013). Breman (2011) also describes lack of experience and socioeconomic standing as hindrances in the effective functioning of political leaders:

"Their problems are manifold: to start with a total ignorance of government programmes and schemes in stock, when and where to circumvent or manipulate rules and regulations, lack of familiarity how to wheel and deal with officials, inability to back up action taken with speed money, i.e. a cash flow 'to get their work done' and last but not least, missing the poise to walk around with confidence in the corridors of the bureaucracy."

In sum, the policy of incentivizing consensus-based elections is controversial, because of its potential to increase the influence of an elite caucus over their community's choice of political representation. As discussed above, it can lead to the appointment of political leaders that merely serve as figureheads. It has also faced criticism from local politicians, who find themselves deprived of financial grants simply because the village chose to have an official election. It is exactly this deprivation that allows the local elite to quash any opposition in the name of obstructing village development, inhibiting the development of leadership in backward areas (Institute of Social Sciences 2012). However, the impact of these financial incentives on electoral competition, politician identity and governance is an open empirical question. To date, there do not exist any studies that quantify the causal impact of these financial grants, a gap that this paper seeks to fill.

Data

Multiple datasets were combined before conducting the empirical analysis. The 2001 and 2011 Population Censuses provides village-level characteristics, including demographic information and public good availability. GP jurisdictions may contain more than one village, and are mapped to villages using the Local Body Mapping data obtained from the Area Profiler website managed by the Ministry of Panchayati Raj.

Information on the 2011 GP elections was obtained from the Gujarat State Election Commission. This includes detailed information on each political candidate for over 75% of GPs, including reservation category, gender, education and occupation. Since these datasets are available only in Gujarati, they were manually merged with the Local Body Mapping dataset described above.

Village level income and expenditure receipts were obtained from the office of the Panchayat, Rural Housing and Rural Development Department. This department manages the Rural Accounts Management System, which keeps track of various categories of expenditure (education, nutrition, villlage development, etc) and income (grants, taxes, fees, etc) at the village budget on an annual basis. This study utilizes village level data for the three fiscal years 2013-16.

Information on the generation of workfare employment under NREGA for 2011-16 was obtained from the NREGA Public Data Portal. Gupta & Mukhopadhyay (2014) show that NREGA's primary implementation constraint is the supply of work generated by GPs, not demand. Therefore, I use information on the amount of employment actually generated as the outcome of interest. This includes measures of the number of households who were provided work, as well as the number of person-days of employment generated each month. To understand whether NREGA targeting changed as a result of the consensus-based elections, I use information on the amount of employment provided to women, Scheduled Castes, Scheduled Tribes and Indira Awaas Yojana (IAY) households.[17]

[17] Indira Awaas Yojana (IAY) is a program targeted at reducing homelessness. IAY households are socioeconomically disadvantaged groups (SCs, STs, free bonded laborers, and other rural households below the poverty line) that receive funding to construct housing units.

3 Empirical Strategy

This section sets up a regression discontinuity design based on the discontinuous increase in financial incentives for "consensus"-based elections at the population threshold 5000. The RD design quantifies the causal impact of the financial incentives on political competition, political selection and government performance.

3.1 Central Specification

I follow the suggestions of Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2008), and use local linear regressions after restricting attention to a close bandwidth around the threshold. Optimal bandwidth choice is based on the procedure outlined in Calonico *et al.* (2014). The identifying assumption is that unobservables vary smoothly at the cutoff.

Let pop_{GP} denote the population under the GP's jurisdiction. For ease of notation, I define a rescaled version of the GP population as $p_g = \frac{pop_{GP}}{1000}$. Restricting attention to observations within the optimal bandwidth, the empirical specification takes the following form:

$$E_{ig} = \gamma + \alpha^0 p_g \mathbb{1}[p_g \leq 5] + \alpha^1 (p_g - 5)\mathbb{1}[p_g > 5] + \beta \mathbb{1}[p_g > 5] + X_g + \epsilon_{ig}$$

E_{ig} denotes an electoral outcome, such as the number of candidates standing for election, for seat i in GP g. This specification includes a constant γ, and fits separate linear regressions before and after the population threshold - the slope coefficient is α^0 before the threshold, and α^1 after the threshold. X_g represents GP-level controls such as the number of villages under the council's jurisdiction as well as demographic controls like the proportion of SC and ST residents. Of primary interest is the β coefficient, which measures discontinuities in E_{ig} as GP population exceeds the policy threshold 5000 (i.e. as g_p exceeds 5). The β estimates are interpreted as the causal effects of financial incentives on electoral competition and political selection. Optimal bandwidths are chosen separately for each outcome, following the procedure prescribed by Calonico *et al.* (2014).[18] Standard errors are clustered at the level of the discrete running variable, GP population.

[18] Optimal bandwidths are estimated using data on GPs with population within a bandwidth of 1000 around the threshold 5000. This avoids the inclusion of other population thresholds at which council composition changes.

Some electoral outcomes are measured at the GP level, like the total number of council seats won without formal opposition. For these outcomes, the central specification uses data at the GP level instead of the seat level:

$$E_g = \gamma + \alpha^0 p_g \mathbb{1}[p_g \leq 5] + \alpha^1 (p_g - 5)\mathbb{1}[p_g > 5] + \beta \mathbb{1}[p_g > 5] + X_g + \epsilon_g$$

The β coefficient measures the causal effect of the grant increase on electoral outcomes, and standard errors are clustered at the GP population level.

Governance

Four outcomes at the GP level are used to study governance - council expenditure, council income with a focus on revenue raised by the local council, and NREGA employment generation and targeting. Annual data on GP income and expenditure is available for the years 2013-16 and data on NREGA implementation is available for the years 2011-16. As there are significant outliers in both sets of data, I trim the top 1% of observations from each of the variables. The empirical specification takes the following form:

$$S_{gy} = \gamma + \alpha^0 p_g \mathbb{1}[p_g \leq 5] + \alpha^1 (p_g - 5)\mathbb{1}[p_g > 5] + \beta \mathbb{1}[p_g > 5] + X_g + \gamma_y + \epsilon_{gy}$$

S_{gy} denotes a governance outcome in GP g in year y, such as income, expenditure or employment creation. This specification includes a constant γ, separate linear regressions before and after the population threshold (α^0 and α^1 denote the slope coefficients before and after respectively), GP level controls X_g and year fixed effects γ_y. The β coefficient measures discontinuities in S_{gy} as GP population exceeds the policy threshold 5000 (i.e. as g_p exceeds 5), and is interpreted as the causal effect of the financial incentives on government functioning. Optimal bandwidths are chosen separately for each outcome, and standard errors are clustered at the level of the discrete running variable, GP population.

Alternative Explanations

As noted previously, two features of the elected council increase discontinuously as GP population exceeds 5000 - the financial incentive for "consensus" elections increases by 50 per cent, and the number of council members increases by 2. This means that the results on electoral and governance outcomes may be driven by the addition of two council members, not the increase in the financial grant. I leverage the existence of alternative population thresholds (i.e. those other than 5000) at which the number of GP members increases by 2. Discontinuity estimates at these alternate thresholds isolate the impact of additional council members on electoral and governance outcomes. These estimates are used to show that it is unlikely that additional council members are driving the effects documented at the threshold 5000.

Panel A of Figure 1 plots the relationship between the number of GP members and population. We can see that the number of council members is fixed at 7 for GPs with population up to 3000, and increases by 2 for every thousand people thereafter. Panel B of Figure 1 plots the relationship between the number of GP seats reserved for women and population. Since the law mandates the reservation of at least 33 per cent of seats for women, the number of seats reserved for women increases by one at every population threshold except 5000 and 8000. Figure A.1 shows that the number of seats reserved for SCs, STs and OBCs does not increase discontinuously at the thresholds 5000 or 8000. Therefore, I estimate the causal impact of two additional council seats (neither reserved for women) by testing for discontinuities in electoral and governance outcomes at the threshold 8000.[19]

The empirical specifications for electoral and governance outcomes (E_{ig} and S_{gy} respectively) are analogous to those described above:

$$E_{ig} = \gamma_8 + \beta_8 \mathbb{1}[p_g > 8] + \alpha_8^0 p_g \mathbb{1}[p_g \leq 8] + \alpha_8^1 (p_g - 8)\mathbb{1}[p_g > 8] + X_g + \epsilon_{ig}$$

$$S_{gy} = \gamma_8 + \beta_8 \mathbb{1}[p_g > 8] + \alpha_8^0 p_g \mathbb{1}[p_g \leq 8] + \alpha_8^1 (p_g - 8)\mathbb{1}[p_g > 8] + X_g + \gamma_y + \epsilon_{gy}$$

where γ_8 is a constant, α_8^0 and α_8^1 are distinct population slopes before and after the threshold 8000, and X_g represents GP-level demographic controls. The β_8 coefficient measures the impact of two additional unreserved seats. Optimal bandwidths are chosen separately for each outcome, following the procedure prescribed by Calonico *et al.* (2014).[20] Standard errors are clustered at the

[19]This implicitly assumes that the interaction effects of the higher incentive and additional members are negligible.

[20]Optimal bandwidths are estimated using data on GPs with population within a bandwidth of 1000 around the threshold 8000. This avoids the inclusion of other population thresholds at which council composition changes.

level of the running variable, GP population.

4 Results

This section presents estimates of the effect of increased financial incentives for "consensus"-based elections. The financial incentive reduces political competition by reducing the number of candidates standing for election for each seat, and increasing the number of seats won without formal opposition at the GP level. The incentive also crowds in a younger, more educated candidate pool; politicians who are ultimately elected from this pool are, on average, 4 years younger and have 2 more years of education. Finally, the impact on multiple measures of governance, including local government expenditure and the targeting of workfare employment, is negative and substantive.

4.1 Baseline Continuity Tests

I first test for evidence of sorting around the population cutoff. This is because GPs may want to be listed as having more than 5000 residents to receive larger samras grant amounts. This is unlikely to be the case, however, because the running variable is taken from the Population Census that was conducted 10 years prior to the introduction of the discontinuous incentives. Figure A.2 presents the density of population surrounding the two cutoffs used in the analysis. Population is collapsed into bins of width 20, and no discontinuity in the vicinity of either of the thresholds is evident. Since the running variable is discrete, I follow Frandsen (2016) to test for the manipulation of reported population close to the cutoffs.[21] The hypothesis of no discontinuity at the threshold 5000 is not rejected at standard significance levels.[22]

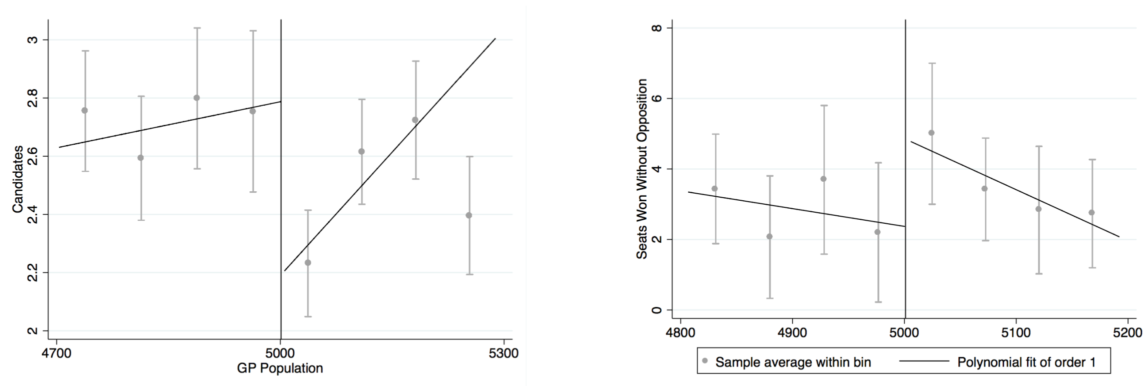
Next, I show that village demographics and public good availability are balanced at baseline (in 2011) by testing for discontinuities at the thresholds 5000 and 8000. These balance tests use information from the 2011 Population Census and are presented in Table A.1. Among the fifty four tests, six yield statistically significant estimates, which is unsurprising and to be expected

[21]The McCrary (2008) test, which is commonly used to test for sorting around thresholds, assumes a continuous running variable. In the case of a discrete running variable, it may falsely reject the null of no manipulation at too high a rate.

[22]Figure A.2 displays p-values from the Frandsen (2016) test for discontinuities at the thresholds 5000 and 8000.

FIGURE 2: ELECTORAL COMPETITION

(A) CANDIDATES PER SEAT **(B) SEATS WON WITHOUT OPPOSITION**



Notes: Figures use data within the optimal bandwidth for each outcome, and display binned means with confidence intervals at the 95% significance level. Results are presented for seats not reserved for women.

mechanically at the 10% level of significance.

Next, I verify that council seats increase at the thresholds 5000 and 8000, while those reserved for women do not. Table A.2 displays discontinuity estimates at each threshold using the central specification. The number of seats increases significantly at both thresholds. Notice that even though there are fewer observations around the threshold 8000, we are still able to reject the hypothesis of no discontinuity. When we repeat the same exercise for the number of seats reserved for women, we do not find evidence of a significant increase at either of the thresholds.

4.2 Electoral Competition

This section presents evidence that the samras grant reduced political competition by disincentivizing multiple candidates from running for each electoral seat. The primary outcome of interest is the number of candidates that stood for election to each council post. I also examine whether the grant increased the number of seats that were won without opposition, i.e. how frequently the grant reduced the number of candidates all the way to one.

First, I examine the impact of the financial grant on political competition by testing for a discontinuous decrease in the number of candidates for each council seat as GP population exceeds 5000. The left panel of Figure 2 shows that the number of candidates for each seat falls by around 0.7 as we cross the threshold, consistent with the hike in incentives for "consensus"-based elections. The left panel of Table 2 displays discontinuity estimates consistent with this graph. The number of candidates per seat falls by 0.7 in response to the samras grant; the decrease in candidates is larger (around 0.9) for seats that are reserved for women. This is a large effect, given that the average number of candidates is just over 2.5.

Since the objective of the grant is to incentivize completely unopposed elections (i.e. to reduce the number of candidates all the way to one), I test whether more seats were won without formal opposition at the population threshold 5000. The right panel of Figure 2 plots the number of seats won without opposition at the GP level, before and after the threshold 5000. Consistent with the hike in incentives for "consensus"-based elections, the number of seats filled without opposition rises by over 2 as we cross the threshold. The right panel of Table 2 displays discontinuity estimates consistent with this graph. The number of seats won without opposition increases by around 2.3 in response to the samras grant. This is a large effect, given that the average number of seats won without opposition is around 3. Further, this decrease is entirely driven by seats that are not reserved for women - unreserved[23] seats won without opposition increase by 2, whereas the coefficient for seats that are reserved for women is small and not significantly different from zero.

TABLE 2: EFFECTS ON ELECTORAL COMPETITION

Outcome	Candidates			Seats Won Without Opposition		
Seat Type	Total	Not Reserved For Women	Reserved For Women	Total	Not Reserved For Women	Reserved For Women
RD Estimate	-0.728**	-0.657**	-0.877**	2.328*	2.092**	0.248
Std. Error	(0.291)	(0.269)	(0.412)	(1.378)	(0.849)	(0.578)
Dep Var Mean	2.553	2.585	2.486	3.130	2.079	1.068
Bandwidth	243.549	253.072	238.180	204.841	203.358	237.051
Observations	2020	1444	616	134	133	158

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level. *** p<0.01, ** p<0.05, * p<0.1

[23]Not reserved for women; may be reserved for other groups.

4.3 Political Selection

This section examines the impact of lower electoral competition on politician identity, as captured by the observable characteristics of elected leaders. These characteristics include age, years of education, gender and primary occupation. The reduction in political competition ushers in younger, more educated representatives, but does not significantly increase the proportion of female politicians. I also test for effects on occupation, and find that the grant does not increase the proportion of candidates from farming and business, which are indicative of elite status within Indian villages (Bhattacharya *et al.* 2016). Therefore, even though qualitative evidence suggests that the local elite have a greater say in "consensus"-based elections, I find suggestive evidence that they do not crowd themselves into political office.

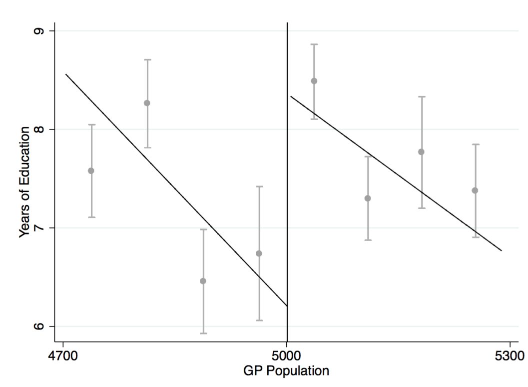
The first set of results shows the impact of the samras grant on age, education, gender and occupational status of candidates for council seats. Table 3 displays discontinuity estimates at the threshold 5000 for all council seats, council seats not reserved for women and council seats reserved for women. The grant crowds in younger, more educated candidates, but does not increase the proportion of female candidates. The proportion of candidates that report either farming or business as their primary occupation (indicative of elite status) does not increase - in fact the estimate is negative, but not significantly different from zero.

All of the above effects are driven by seats that are not reserved for women. This is consistent with the finding that the number of female-reserved seats won without opposition does not increase in response to the grant. Among seats that are not reserved for women, candidate age decreases by 3 years and years of education increases by 1.9. Despite the additional incentives for female candidates, I do not find any evidence that the samras grant crowded female representatives into seats not reserved specifically for them. The top panel of Figure 3 displays the discontinuous change in candidate age and educational achievement for unreserved seats graphically.

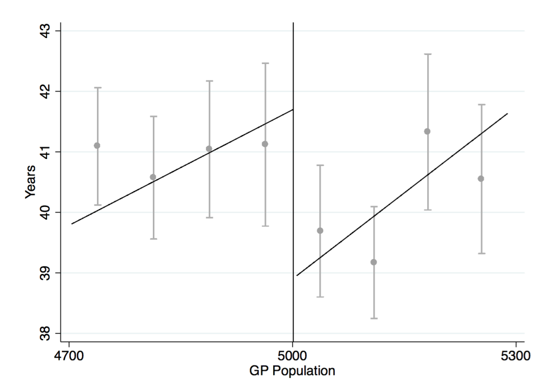
Next, I examine whether changes in the candidate pool translated into a change in politician identity. Table 4 repeats the above analysis, but for politician (i.e. eventual winner) characteristics instead of candidate characteristics. Politicians are significantly younger, by an average of over 4

FIGURE 3: CANDIDATE & POLITICIAN SELECTION EFFECTS

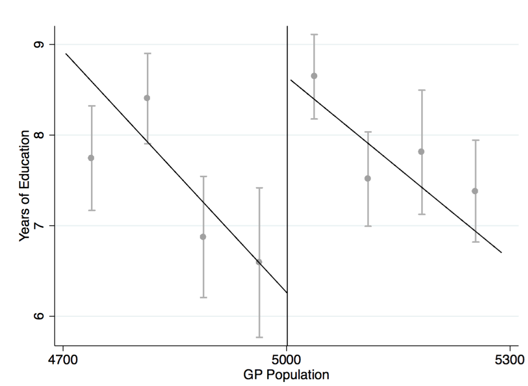
(A) CANDIDATE EDUCATION



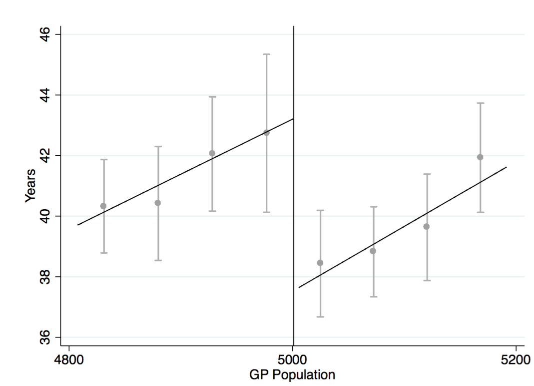
(B) CANDIDATE AGE



(C) WINNER EDUCATION



(D) WINNER AGE



Notes: Figures use data within the optimal bandwidth for each outcome, and display binned means with confidence intervals at the 95% significance level. Results are presented for seats not reserved for women.

TABLE 3: EFFECTS ON CANDIDATE POOL

All Seats

Outcome	Education (Years)	Age (Years)	Female	Occupation Farming/Business	Job	Ag. Labor
RD Estimate	1.924***	-2.383**	-0.033	-0.089	0.021	0.020
Std. Error	(0.738)	(1.198)	(0.025)	(0.065)	(0.013)	(0.045)
Dep Var Mean	6.917	39.862	0.361	0.569	0.017	0.129
Bandwidth	258.203	239.669	333.544	340.671	267.425	238.796
Observations	2257	2093	2768	2818	2326	2086

Seats Not Reserved for Women

Outcome	Education (Years)	Age (Years)	Female	Occupation Farming/Business	Job	Ag. Labor
RD Estimate	1.907***	-2.996***	0.006	-0.074	0.027	0.030
Std. Error	(0.634)	(1.131)	(0.035)	(0.065)	(0.017)	(0.047)
Dep Var Mean	7.696	40.573	0.084	0.658	0.024	0.138
Bandwidth	284.694	295.744	395.083	404.814	256.101	295.369
Observations	1691	1761	2310	2380	1549	1761

Seats Reserved for Women

Outcome	Education (Years)	Age (Years)	Occupation Farming/Business	Job	Ag. Labor
RD Estimate	1.511	-0.992	-0.160	0.008	0.016
Std. Error	(1.055)	(2.002)	(0.122)	(0.006)	(0.059)
Dep Var Mean	5.203	38.298	0.373	0.002	0.111
Bandwidth	253.434	234.164	236.590	195.774	225.881
Observations	678	629	637	530	608

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level. *** p<0.01, ** p<0.05, * p<0.1

years, and educational attainment rises by around 1.7 years. Effects are only found for politicians elected to seats not reserved for women, results that are displayed graphically in Figure 3. Despite the fact that the samras scheme offered additional incentives for female politicians, the proportion of female politicians does not increase. There does not seem to be a substantive effect of the samras grant on politician occupation either.

"Consensus"-based elections could crowd in younger, more educated politicians for two reasons. First, the majority of rural residents may consider these characteristics to be desirable for an effective political leader, and public deliberation helps shift candidates with these characteristics into political office. Under this hypothesis, we would expect to see GPs that face higher samras grants enjoying better governance than GPs that face lower grants. Second, local elites may nominate younger, inexperienced candidates that serve as political figureheads. This explanation is consistent with survey evidence that the grant amount is only used to justify nominations by the local elite, who threaten detractors in the name of village development. Under this scenario, we would not expect to see governance improve. In order to separate between the two hypotheses and determine whether "consensus"-based elections have had a beneficial impact on governance, I turn to four measures of the performance of the elected council.

4.4 Governance

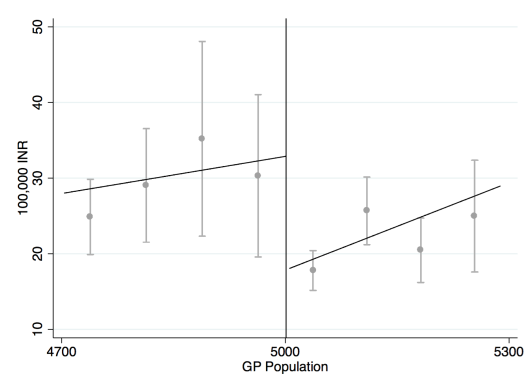
This section shows that GPs that faced higher samras grants differ systematically in terms of governance. Total expenditure in the GP is significantly lower, and this decrease is driven by expenditure categories that are directly controlled by the elected council. The targeting of workfare employment, a direct responsibility of the elected council, also worsens.

4.4.1 GP Income and Expenditure

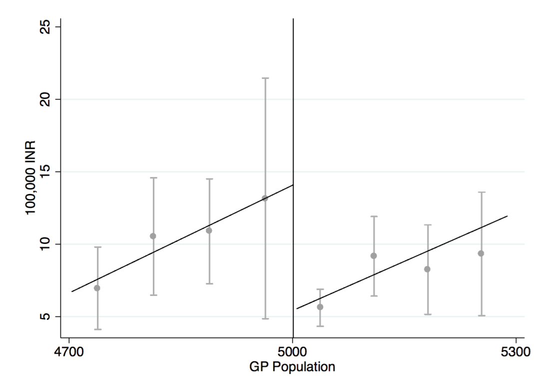
I first examine effects of the samras grant on GP expenditure and its components. The first two columns of Table 5 display estimates of discontinuities in total and "own fund" expenditure as GP population exceeds 5000. Own fund expenditure refers to all expense categories that are decided upon by the elected council such as program expenses of the agriculture, education and health departments, as well as salaries and other administrative expenses. The elected council has limited influence over the remaining expense categories, since these are chosen by and tied to grants received from the state and central government. The first column shows that total expenditure decreases significantly and substantively - by over half of the mean. The second column shows that this decrease is driven by a significant drop in own fund expenses. That is, spending on local administration and development of the village falls as well. The upper panel of Figure 4 displays

Figure 4: GP Expenditure & Income

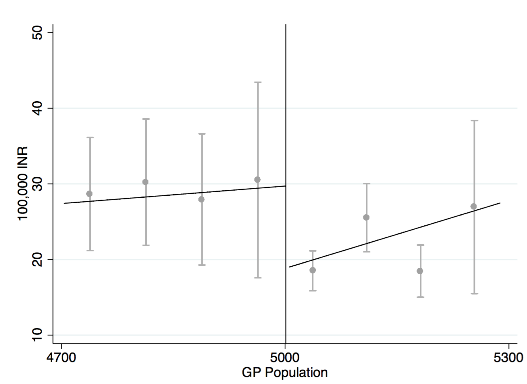
(a) Total Expenditure



(b) Own Fund Expenditure



(c) Total Income



(d) Own Fund Income

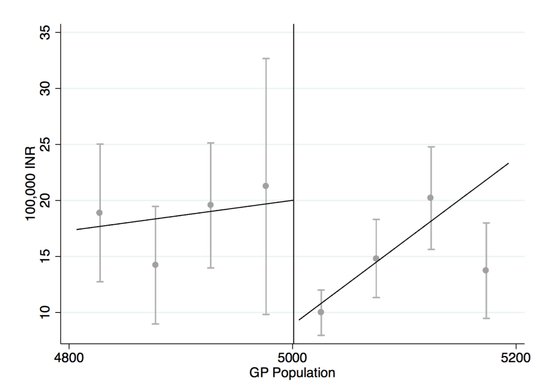


TABLE 4: EFFECTS ON POLITICIAN IDENTITY

All Seats

Outcome	Education (Years)	Age (Years)	Female	Occupation: Farming/Business	Job	Ag. Labor
RD Estimate	1.653**	-4.299**	-0.024	-0.066	0.007	-0.016
Std. Error	(0.699)	(1.720)	(0.031)	(0.066)	(0.016)	(0.055)
Dep Var Mean	7.014	39.767	0.357	0.579	0.016	0.124
Bandwidth	354.939	195.636	240.019	358.748	284.026	234.337
Observations	2773	1644	2006	2847	2316	1946

Seats Not Reserved for Women

Outcome	Education (Years)	Age (Years)	Female	Occupation: Farming/Business	Job	Ag. Labor
RD Estimate	2.122***	-5.660***	0.042	-0.106	0.008	0.002
Std. Error	(0.721)	(1.520)	(0.038)	(0.081)	(0.023)	(0.050)
Dep Var Mean	7.828	40.502	0.066	0.674	0.023	0.134
Bandwidth	314.098	224.284	218.639	307.208	281.295	273.452
Observations	1723	1293	1248	1697	1571	1536

Seats Reserved for Women

Outcome	Education (Years)	Age (Years)	Occupation: Farming/Business	Job	Ag. Labor
RD Estimate	0.829	-1.881	-0.147	0.000	-0.029
Std. Error	(1.079)	(2.799)	(0.134)	(0.000)	(0.085)
Dep Var Mean	5.283	38.203	0.377	0.001	0.101
Bandwidth	319.066	224.893	227.502	215.847	227.946
Observations	808	587	587	561	587

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

these discontinuity estimates graphically.

Is a decrease in council income driving the negative coefficients on expenditure? The third column of Table 5 shows that total income does not decrease significantly as population exceeds the threshold 5000. This estimate is negative, but not significantly different from zero and much smaller in magnitude than the fall in expenditure. As discussed previously, councils receive grants from state and national governments, but also collect taxes within their jurisdiction. Since political experience and socioeconomic standing may aid in the generation of government revenue, I examine whether "own fund" revenue changes discontinuously as population exceeds 5000. Own fund revenue includes revenue generated through the collection of taxes, fees and other charges. The last column of Table 5 shows that the effect on own fund revenue is negative and sizable (around 45 per cent of the mean), but not statistically significant. The lower panel of Figure 4 displays these discontinuity estimates graphically.

TABLE 5: EFFECTS ON INCOME AND EXPENDITURE

Outcome	Expenditure			Income		
	Total	Own Fund	Grant	Total	Own Fund	Grant
RD Estimate	-12.13*	-4.222*	1.000	-3.621	-6.474	-1.511
Std. Error	(7.006)	(2.424)	(2.333)	(4.763)	(5.653)	(2.274)
Dep Var Mean	21.522	6.858	11.826	20.903	14.110	5.769
Bandwidth	219.733	213.769	314.766	254.541	236.877	268.096
Observations	395	384	540	455	421	475

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level. *** p<0.01, ** p<0.05, * p<0.1

4.4.2 Employment Creation Under NREGA

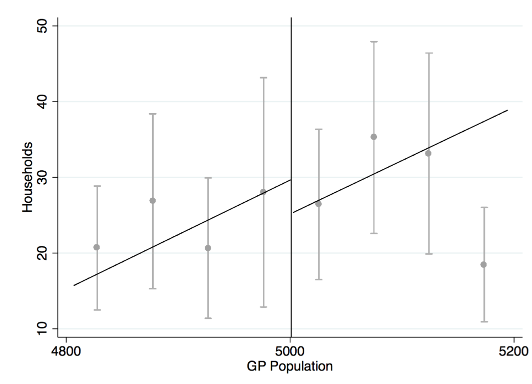
The National Rural Employment Guarantee Act is intended to guarantee one hundred days of employment to each rural household, in order to complement market demand for labor and provide income insurance for families close to the poverty line. In practice, the amount and targeting of work provided is left up to local implementing authorities, and heavily influenced by the elected GP (Gupta & Mukhopadhyay 2014). Since the Act is entirely funded by the central government, the GP's main role is to formulate plans for worksites based on the needs of the village, petition for funding from higher level authorities and choose program beneficiaries.

I examine four measures of annual employment generation by the council in Table 6 - the number of households employed, how many of these are SC, ST and IAY households, the number of person-days of employment generated, and how many of these are allocated towards SCs, STs and women.[24] The effect of the samras grant on overall employment creation is negative but not

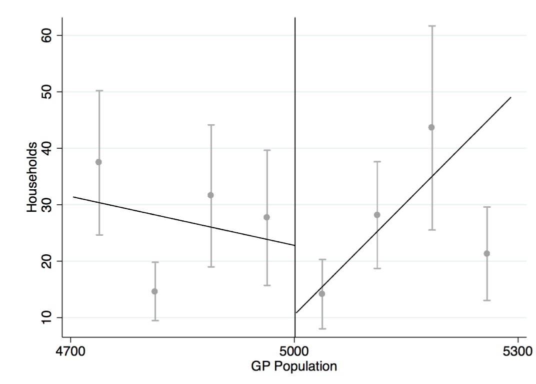
[24]The NREGA implementation data does not contain information on the number of women-only households pro-

FIGURE 5: WORKFARE EMPLOYMENT: CREATION AND TARGETING

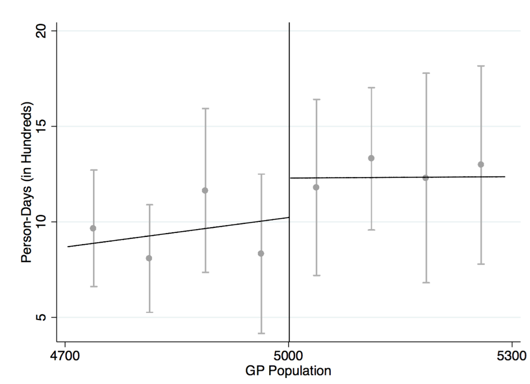
(A) NON-SC/ST HOUSEHOLDS



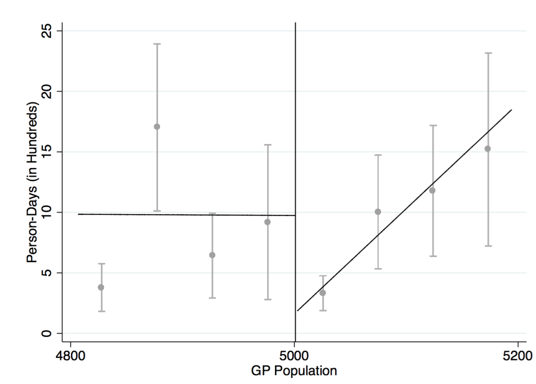
(B) SC/ST HOUSEHOLDS



(C) NON-SC/ST PERSON-DAYS



(D) SC/ST PERSON-DAYS



significantly different from zero. Targeting, however, is clearly negatively affected - the number of SC and ST households and person-days decrease discontinuously. The estimates are large - over fifty per cent of the mean in each case - and statistically different from zero. Figure 5 displays graphical estimates for a subset of these outcomes.

4.5 Ruling Out Alternative Explanations

To ensure that these results are not driven by either the change in council size or council reservations, I repeat the above exercises for the population threshold 8000. The number of council seats increases discontinuously at the threshold 8000, while the number of seats reserved for women

vided employment, or person-days allocated towards IAY beneficiaries.

TABLE 6: EFFECTS ON EMPLOYMENT CREATION AND TARGETING

	Households Employed			
Category	Total	Scheduled Caste	Scheduled Tribe	Land Reform
RD Estimate	-19.82	-4.399**	-11.180*	-0.228
Std. Error	(15.206)	(2.089)	(6.360)	(0.152)
Dep Var Mean	55.726	3.042	21.885	0.412
Bandwidth	188.963	213.295	278.014	165.483
Observations	620	728	943	573

	Person-Days of Employment			
Category	Total	Scheduled Caste	Scheduled Tribe	Women
RD Estimate	-7.056	-1.408*	-6.414**	1.088
Std. Error	(6.694)	(0.760)	(3.149)	(2.782)
Dep Var Mean	21.789	1.224	7.818	9.547
Bandwidth	189.912	231.675	257.836	372.755
Observations	619	779	870	1213

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

does not. Moreover, the samras grant does not change at this threshold. Therefore, discontinuity estimates at the threshold 8000 represent the effect of increasing council size alone. I find that the pattern of results documented at the threshold 5000 are not present at the threshold 8000. That is, it is likely that the samras grant is driving the reduction in political competition, change in politician identity and worsening of governance.

Estimates of the effect of council size on electoral competition are presented in Table A.3. None of the coefficients are statistically significant[25] and the coefficient measuring the drop in the number of candidates per seat is less than 0.1. Tables A.4 and A.5 present estimates of the effect of council size on candidate and politician characteristics. None of the coefficients are statistically

[25]Table A.2 shows that this cannot be attributed to the fact that there are fewer observations around the threshold 8000. Despite the smaller number of observations, we can strongly reject the hypothesis that the number of council members does not increase at the threshold 8000.

significant, and the coefficient on age in particular is much smaller in magnitude than those found at the threshold 5000.

Turning to measures of government performance, Table A.6 shows that the pattern of results found at the threshold 5000 is not replicated. The impact of council size on overall and own fund expenditure is negative but not significantly different from zero. Instead, the coefficient on total income is negative and statistically significant. The decrease in own fund income remains statistically indistinguishable from zero. Finally, Table A.7 presents estimates of the impact of council size on employment creation and targeting under NREGA. I find that an increase in council size alone cannot explain the effects found at the threshold 5000, since none of the coefficients are statistically significant and many are positive, which is the opposite of the effect documented at the threshold 5000. Altogether, these results indicate that the reduction in political competition, crowding in of younger, more educated politicians and worsening of governance is due to the increase in the "consensus" election grant.

5 Conclusion

This paper quantifies the impact of encouraging the selection of political representatives via community consensus, as opposed to a secret ballot election, on both the pool of candidates and politicians that are eventually elected. It also examines the effects of encouraging such elections on multiple measures of governance. The analysis makes use of a novel dataset containing detailed information on candidates, politicians and government functioning at the village level in the state of Gujarat in India for the period 2011-15.

To retrieve causal estimates, I exploit the existence of a population threshold at which financial incentives for "consensus"-based elections sharply increase. The results indicate that financial incentives can induce village electorates to choose their political leaders without formal opposition. I also find that the reduction in competition crowds in younger, more educated candidates and politicians. Finally, I study four measures of governance over which the elected council exercises substantial influence, and find a significant reduction in total expenditure as well as an increase in how regressively workfare employment is targeted. These findings are consistent with the fact that politicians that rely on the support of local elites, who have a greater say in elections based on community "consensus", are not incentivized to appease the majority of village residents.

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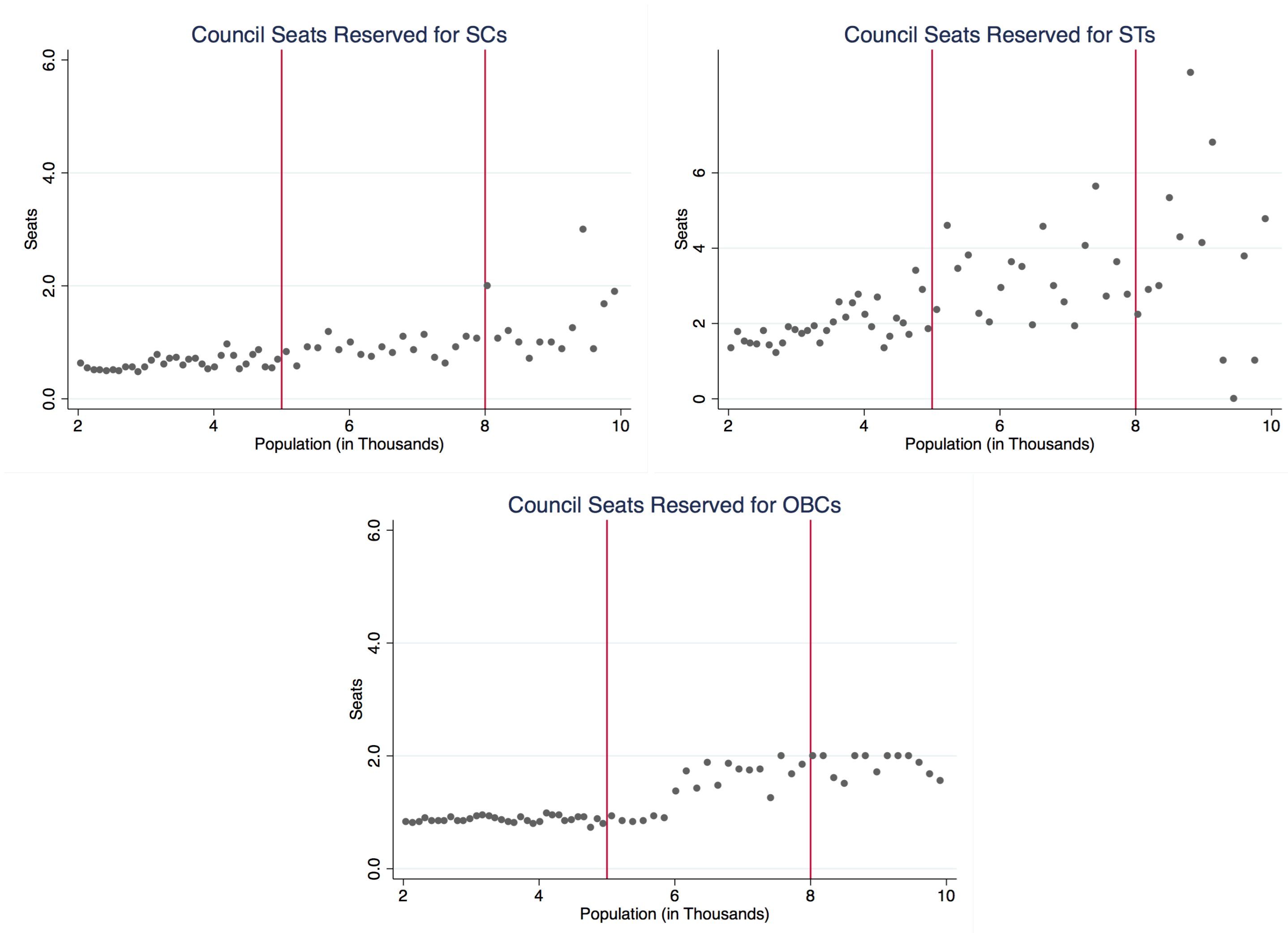
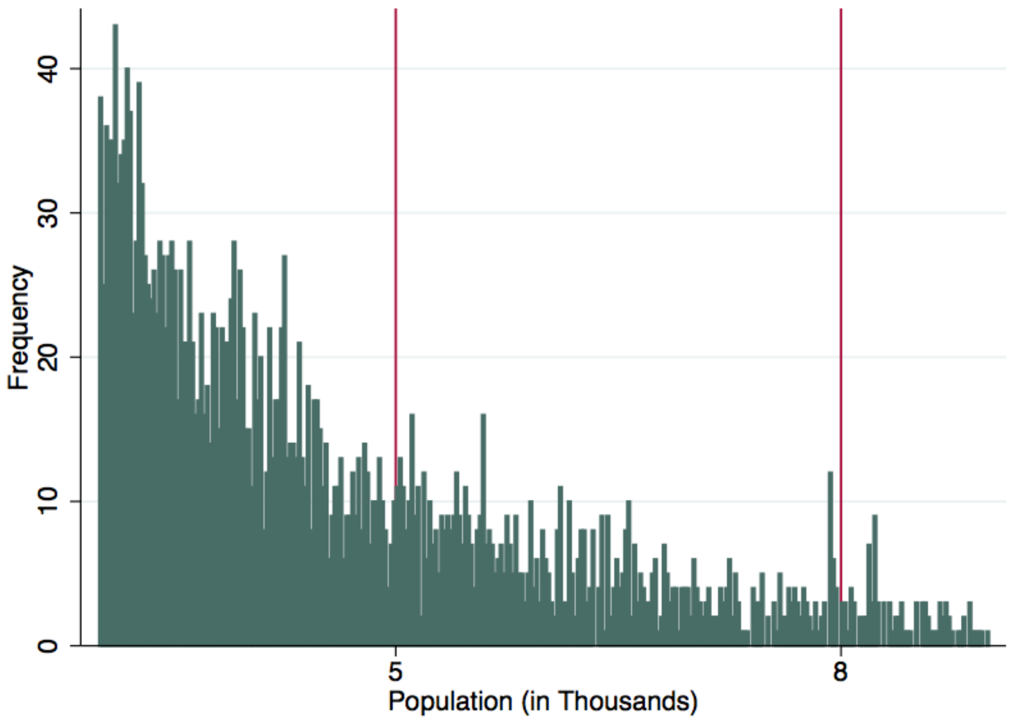
Appendix

FIGURE A.1: COUNCIL MEMBER RESERVATIONS & GP POPULATION



Notes: GP Seats reserved for Scheduled Castes, Scheduled Tribes and Other Backward Castes do not increase discontinuously at the population thresholds 5000 and 8000.

FIGURE A.2: DISTRIBUTION OF GP POPULATION



Notes: Population is grouped into bins of 20. The Frandsen (2016) RD Density Test does not reject the hypothesis of continuity in the population distribution at the thresholds 5000 and 8000 (p-values are 0.2 and 0.82 respectively).

Table A.1: Baseline Covariates: Demographics & Public Goods

Characteristic	β	Bandwidth	β_8	Bandwidth
Number of Households	80.02	293	-5.74	271
	(82.99)		(220.32)	
Male Population	27.61	378	660.28	245
	(183.15)		(637.84)	
Female Population	-1.18	393	622.58	251
	(172.36)		(654.24)	
Scheduled Caste Male Population	-1.1	310	100.00	268
	(42.36)		(93.59)	
Scheduled Caste Female Population	-0.75	310	109.76	264
	(38.74)		(87.45)	
Scheduled Tribe Male Population	-133.67	273	-88.74	414
	(320.88)		(845.92)	
Scheduled Tribe Female Population	-135.19	271	-97.49	416
	(319.19)		(857.73)	
Geographic Area	-276.27	371	1554.14	371
	(438.68)		(994.85)	
Pre-Primary Schools	-0.05	252	-0.26	405
	(0.04)		(0.35)	
Primary Schools	0.46	263	2.51	300
	(0.71)		(1.67)	
Secondary Schools	0.03	399	-0.08	348
	(0.19)		(0.35)	
Senior Secondary Schools	0.26	397	-0.93	273
	(0.14)		(0.42)	
Tap Water	0.05	249	0.01	227
	(0.09)		(0.07)	
Covered Wells	0.05	266	-0.13	246
	(0.13)		(0.19)	
Uncovered Wells	0.04	331	-0.02	239
	(0.15)		(0.31)	
Handpumps	0.21	254	0.13	262
	(0.12)		(0.29)	
Tubewells & Borewells	0.03	438	-0.29	176
	(0.12)		(0.32)	
Springs	-0.01	220	0.01	339
	(0.02)		(0.18)	
Rivers & Canals	0.09	186	-0.11	269
	(0.16)		(0.28)	
Tanks, Ponds & Lakes	0.21	318	-0.11	245
	(0.13)		(0.28)	
Closed Drainage	0.07	283	-0.22	204
	(0.12)		(0.16)	
Open Drainage	0.04	393	-0.10	292
	(0.13)		(0.25)	
Community Toilet Complex	-0.03	325	0.19	346
(Including Baths)	(0.02)		(0.15)	
Community Toilet Complex	0.04	301	-0.10	300
(Excluding Baths)	(0.10)		(0.22)	
Rural Production Centres	-0.04	290	0.26	318
	(0.06)		(0.28)	
Community Waste Disposal System	0.08	262	-0.31	230
	(0.09)		(0.33)	
Community Biogas/Waste Recycling	0.04	373	-0.04	409
	(0.07)		(0.16)	

Notes: Outcome variables are based on the 2011 Census. Optimal bandwidths are chosen separately for each characteristic, and robust standard errors are based on Calonico *et al.* (2014).

TABLE A.2: RULE BASED EFFECTS: COUNCIL SIZE AND RESERVATIONS FOR WOMEN

Threshold	5000		8000	
Outcome	Total Seats	Seats Reserved For Women	Total Seats	Seats Reserved For Women
RD Estimate	2.160	0.317	3.682	1.090
Std. Error	(0.529)	(0.313)	(1.565)	(0.695)
Dep Var Mean	11.144	3.983	16.311	5.571
Bandwidth	264.930	318.599	390.535	402.394
Observations	703	703	161	161

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.

TABLE A.3: EFFECTS OF ADDITIONAL MEMBERS ON ELECTORAL COMPETITION

Outcome	Candidates			Seats Won Without Opposition		
Seat Type	Total	Not Reserved For Women	Reserved For Women	Total	Not Reserved For Women	Reserved For Women
RD Estimate	-0.0724	0.0514	0.0442	2.981	1.816	1.331
Std. Error	(0.451)	(0.436)	(0.449)	(3.139)	(2.028)	(1.233)
Dep Var Mean	2.643	2.747	2.415	4.423	2.877	1.546
Bandwidth	193.715	199.071	285.451	279.434	293.965	248.808
Observations	668	486	300	55	58	51

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.

TABLE A.4: EFFECTS OF ADDITIONAL MEMBERS ON CANDIDATE POOL

Outcome	Education (Years)	Age (Years)	Female	Occupation		
				Farming/Business	Job	Ag. Labor
RD Estimate	1.791	-0.122	0.157	0.024	-0.072	-0.016
Std. Error	(1.659)	(0.972)	(0.128)	(0.153)	(0.046)	(0.073)
Dep Var Mean	7.277	39.918	0.363	0.531	0.030	0.100
Bandwidth	173.924	177.974	207.185	366.124	261.097	231.093
Observations	604	604	760	1211	1028	904

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.

TABLE A.5: EFFECT OF ADDITIONAL MEMBERS ON POLITICIAN IDENTITY

Outcome	Education (Years)	Age (Years)	Female	Occupation		
				Farming/Business	Job	Ag. Labor
RD Estimate	1.977	-0.978	0.139	0.047	-0.099	0.014
Std. Error	(1.984)	(1.618)	(0.128)	(0.166)	(0.054)	(0.067)
Dep Var Mean	7.362	39.775	0.362	0.548	0.028	0.088
Bandwidth	172.861	137.907	209.296	310.065	227.998	249.290
Observations	552	476	708	1020	800	886

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.

TABLE A.6: EFFECTS OF ADDITIONAL MEMBERS ON INCOME AND EXPENDITURE

Outcome	Expenditure			Income		
	Total	Own Fund	Grant	Total	Own Fund	Grant
RD Estimate	-15.37	-7.279	-8.554	-20.34	-8.138	-3.660
Std. Error	(12.419)	(5.913)	(6.613)	(11.067)	(8.950)	(4.308)
Dep Var Mean	32.014	12.830	15.278	33.020	22.525	8.031
Bandwidth	168.987	181.012	172.008	165.629	169.518	268.743
Observations	69	78	69	69	70	134

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.

TABLE A.7: EFFECTS OF ADDITIONAL MEMBERS ON EMPLOYMENT CREATION

	Households Employed			
Category	Total	Scheduled Caste	Scheduled Tribe	Land Reform
RD Estimate	-4.934	0.355	6.146	-0.274
Std. Error	(30.444)	(2.883)	(16.785)	(0.253)
Dep Var Mean	47.520	2.152	16.222	0.477
Bandwidth	328.771	213.385	392.632	231.497
Observations	338	220	385	254

	Person-Days of Employment			
Category	Total	Scheduled Caste	Scheduled Tribe	Women
RD Estimate	2.436	0.145	-0.412	-0.474
Std. Error	(13.005)	(0.886)	(3.824)	(5.820)
Dep Var Mean	18.246	0.733	5.570	8.142
Bandwidth	308.807	240.439	305.111	380.111
Observations	318	266	318	373

Notes: Local linear regressions, triangular kernel; standard errors clustered at the GP population level.